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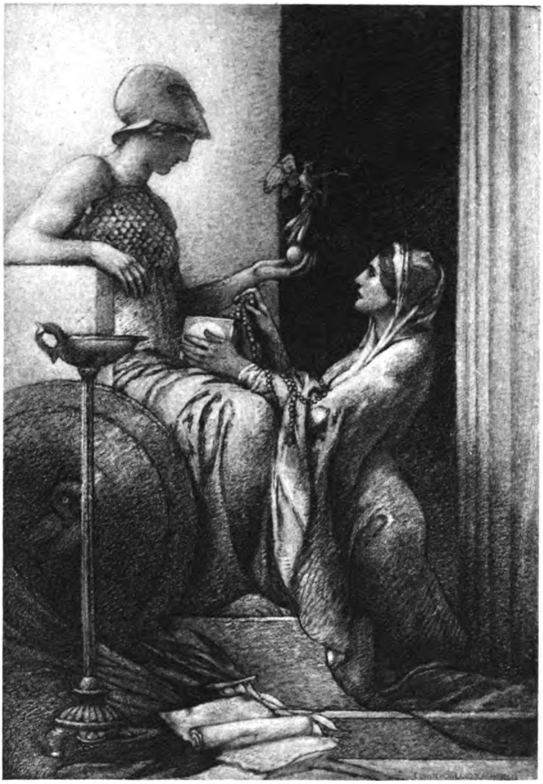

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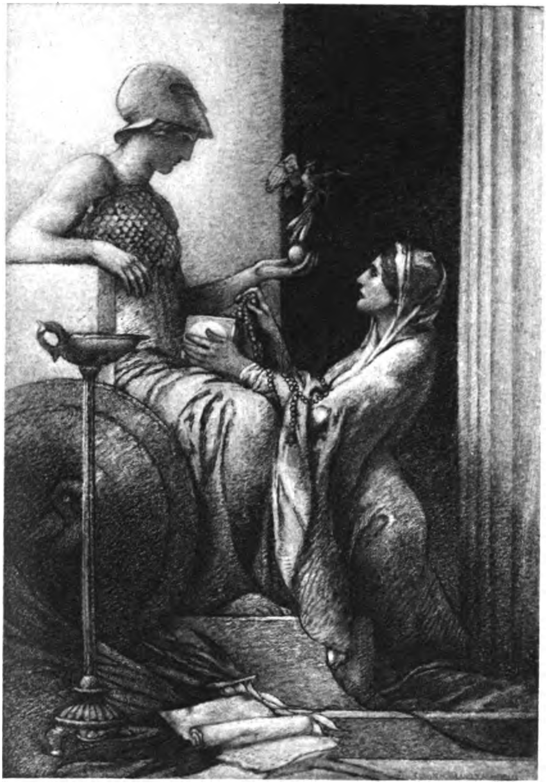

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THE
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VOL. XXXIII.

For JANUARY, FEBRUARY, MARCH, and APRIL, 1809.

LONDON:
PRINTED BY RICHARD TAYLOR AND CO., SHOE LANE:
And sold by RICHARDSONS; CADELL and DAVIES; LONGMAN, HURST,
REES, and ORME; VERNOR, HOOD, and SHARPE; MURRAY;
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THE
PHILOSOPHICAL MAGAZINE.

I. *Result of some Experiments on the Distillation of various Vegetable and Animal Substances in the dry Way.* *By* DAVID MUSHET, *Esq.*

THE following are some of the experiments promised in my last communication to the Philosophical Magazine*.

Experiment I.

Raw Sugar.—270 grains, being distilled till all volatile matter was separated, were found to have been reduced in weight to 38 grains, having lost 232 grains.

100 parts therefore contained: Volatile matter 85·9
Oxide of carbon 14·1

The charcoal obtained in this experiment was light and spongy, and possessed the most fascinating prismatic colours; gold, purple, blues, and indigos. It occupied about three times the bulk it possessed when raw.

Experiment II.

Loaf Sugar, single refined, 264 grains.—This in distillation also swelled greatly in bulk, and discharged a pure white flame edged with blue. The coal had in forming entered into complete fusion, and resembled the former product, though not possessed of the same brilliant shades.—It weighed 39 grains. Loss by distillation 225 grains.

Component parts of loaf sugar: Volatile matter 85·26
Oxide of carbon 14·74

100 parts.

From these experiments it is evident that sugar possesses similar properties with some varieties of pit-coal, in so far as it cakes or welds in distillation. (See Phil. Mag. vol. xxxii.)

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* See Vol. xxxii.

Experiment III.

White Silk Stuff, 200 grains.—These were distilled with a violent discharge of bituminous flame and smoke. The residuum was formed into a caked coal of a dense compact structure; cellular, though not in the least brittle. The colour was iron-gray, with a faint shade of copper. When struck, it was sonorous in a great degree.—It weighed 62 grains. Loss in distillation 138 grains.

Component parts:	Volatile matter	69
	Oxide of carbon	31
		100 parts.

Experiment IV.

Pure White Wool, 180 grains.—After being distilled, there was found a residuum coal of a dark shining gray colour, welded, or rather caked into one mass, adhering in part to the sides of the retort.—It was found to weigh 43 grains. Loss in distillation 137 grains.

Component parts:	Volatile matter	76·11
	Oxide of carbon	23·89
		100 parts.

Experiment V.

Rice, very clean, 240 grains.—This substance distilled with a good deal of flame. The result was found to be a dense coal that had entered completely into fusion, and possessed no remains of the original grains of rice.—It now weighed 42 grains. Loss by distillation 198 grains.

Component parts:	Volatile matter	82·5
	Oxide of carbon	17·5
		100 parts.

Experiment VI.

Barley, well dried, 200 grains.—This flamed violently, and burnt like coal or fat wood. The result was a firmly welded mass of the same shape with the interior of the retort. Each grain of barley preserved its original form, but firmly welded to each other by a porous cement of silvery-coloured coal, which in some places was prismatic.—It weighed 38 grains. Loss in distillation 162 grains.

Component parts: Volatile matter 81
Oxide of carbon 19
100 parts.

Experiment VII.

Eggs.—An egg was boiled hard and the shell taken off, the core was then allowed to dry till it had attained the consistency of a horse's hoof, which it greatly resembled. In this state it was found to weigh 333 grains. In distillation it yielded a great quantity of white flame of a dazzling colour. The residuum had passed by fusion into a silvery bright porous coal that weighed 21 grains. Loss by distillation 312 grains.

Component parts: Volatile matter 93·7
Oxide of carbon 6·3
100 parts.

Experiment VIII.

Goose Feathers, 107 grains.—This substance in distilling yielded at first a heavy smoke, which was afterwards succeeded by a pale blue flame. The residuum coal was uncommonly light and spongy. It had caked into one mass during the exposure, and now weighed 11 grains, having lost in distillation 96 grains.

Component parts: Volatile matter 90
Oxide of carbon 10
100 parts.

Experiment IX.

Cork, in small pieces, 200 grains.—These united together and swelled into three times their original volume, similar to a caking coal. The charcoal obtained from this substance, when pounded, was nearly ten times the bulk of

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common vegetable charcoal. The mass was found to weigh 32 grains. Loss by distillation 168 grains.

Component parts: Volatile matter 84
Oxide of carbon 16

100 parts.

Experiment X.

Horse-Hoof, 180 grains.—The coal afforded by the distillation of hoof was light, spongy, and of a silver-gray colour. It was formed by a perfect fusion of the pieces into one mass, and weighed 24 grains. Loss by distillation 156 grains.

Component parts: Volatile matter 86·6
Oxide of carbon 13·4

100 parts.

Experiment XI.

White Horse Hair, 202 grains.—This operation was carried on with a small discharge of flame. The hair was found resolved by fusion into a light porous coal, of a silvery-gray colour, which weighed 28 grains, having lost by distillation 174 grains.

Component parts: Volatile matter 86·13
Oxide of carbon 13·87

100 parts.

Experiment XII.

Black Horse Hair, 200 grains yielded by distillation a very fine coal largely honeycombed, and of a silvery-gray colour, weighing 38 grains, having lost in distillation 162 grains.

Component parts: Volatile matter 81
Oxide of carbon 19

100 parts.

A portion of hair taken from the mane of the same horse, yielded of oxide of carbon 25·14 per cent.

The subjects of these experiments all afford coal, which partakes of the nature of welding or caking pit-coal, and most of them in distillation exhibited similar phænomena.

The substances operated upon in the following experiments yield a carbonaceous residuum, more of the nature of wood, in so far as the original masses are seldom, or but slightly, altered in shape or appearance during the operation of distilling.

Experiment XIII.

Swedish Turnip that had remained in the ground during the winter, washed clean, and séparated from the skin, 500 grains, emitted during distillation a strong smell of vegetable matter, and towards the close of the operation a minute portion of flame. A fine prismatic coal was obtained which weighed 20 grains.—Loss by distillation 480 grains.

Component parts:	Volatile matter	96
	Oxide of carbon	4
		100 parts.

The skins were distilled in a similar manner, and with a similar result as to coal. The product was only equal to 3·4 parts in 100.

Experiment XIV.

Potatoe.—A well washed potatoe dried, weighed 697 grains. After distillation, a beautiful prismatic coal was found. The original shape of the potatoe was still preserved, but the internal structure was materially changed; a regular arrangement of delicately coloured cavities had succeeded the vegetable organization, and the whole considerably resembled a honeycomb. The product was found to weigh 36 grains, having lost in distillation 661 grains.

Component parts:	Volatile matter	94·83
	Oxide of carbon	5·17
		100 parts.

Experiment XV.

Garden Beans, 330 grains.—In distillation these yielded a small portion of grayish blue flame. The beans were found possessed of the same shape as when first introduced. They were quite detached from each other, contained many fine prismatic shades upon their surface, and weighed 40 grains. Loss in distillation 290 grains.

Component parts: Volatile matter 87·8
Oxide of carbon 12·2
100 parts.

Experiment XVI.

Common Field Pease, 240 grains, exhibited the same appearances both during and after the distillation, and yielded of prismatic coal 48 grains, having lost in distillation 192 grains.

Component parts: Volatile matter 80
Oxide of carbon 20
100 parts.

Experiment XVII.

Oats deprived of their husk, 240 grains.—A considerable portion of whitish blue flame was disengaged during the distillation. The grains were found in the state of coal, of a black coppery colour, free, and possessed of their original shape.—Weight of the coal 50 grains. Loss in distillation 190 grains.

Component parts: Volatile matter 79·16
Oxide of carbon 20·84
100 parts.

Experiment XVIII.

Flax, 397 grains.—After a copious discharge of white flame, a soft inflammable coal was found. The original vegetable fibre was entire, and equally compact as to shape and bulk as when first introduced.—It weighed 65 grains, having lost in distillation 332 grains.

Component parts: Volatile matter 83·62
Oxide of carbon 16·38
100 parts.

Experiment XIX.

Cotton Cloth, well bleached, 263 grains.—A considerable portion of pale blue flame was discharged during the operation of distilling. A light friable coal was obtained, possessed of the original shape and texture of the cloth.—It was found to weigh 39 grains. Loss in distillation 124 grains.

Component parts:	Volatile matter	85·16
	Oxide of carbon	14·84
		100 parts.

Experiment XX.

English Apple, cut into square pieces, 620 grains.—The distillation of this substance afforded but a small portion of flame towards the close of the operation. A light prismatic coal was obtained. The shades chiefly blues with orange, and so vivid as to bear an intimate resemblance to the same colours upon polished steel. The individual masses were much shrivelled, but not in the least adhering together.—Weight 20 grains. Loss by distillation 600 grains.

Component parts:	Volatile matter	96·77
	Oxide of carbon	3·23
		100 parts.

Experiment XXI.

Cinnamon, 150 grains.—This substance yielded a small portion of white flame edged with pale blue. The coal was partially prismatic, but the shades were few, and dull in point of colour. It possessed no symptoms of welding or caking, and weighed 36 grains, having lost by distillation 114 grains.

Component parts:	Volatile matter	76
	Oxide of carbon	24
		100 parts.

Experiment XXII.

Sweet Almonds, 240 grains.—These in distilling discharged a great quantity of oily flame resembling the product of pit-coal. The almonds were found entire, and in the state of a prismatic coal.—Their weight was 33 grains. Loss in distillation 207 grains.

Component parts:	Volatile matter	86·25
	Oxide of carbon	13·75
		100 parts.

[To be continued.]

II. *Description of a Portable Bridge, invented by Mr.* JAMES ELMES, *Architect, of College-Hill, Queen-Street, Cheapside, London**.

BRIDGES upon this construction may be rendered either permanent or otherwise. The only difference will be, that for the former the parts may be strongly bolted and fastened together, instead of being joined by contrivances which admit of the parts being separated, for the convenience of removal, as in the drawing now sent. (See Plate I.)

The component parts shall be first described, and afterwards the manner of applying them. A is a strong iron frame that forms the bottom. B is a square frame of the same metal, fastened by hinges, to the ends of A, for the purpose of falling down flat upon the bottom for conveniency of packing, as shown by the figure C. A skirting of iron plate marked D, is also strongly fixed to the bottom, as in the elevation of the whole P, and in the figure B. Two spring catches are attached to this skirting to keep the sides steady when erected. One of these catches E on a larger scale is shown in the drawing. The remaining detached parts are marked F and G. F is a square iron link separated in the middle, and each part opening by a spring. G is a kind of staple opening and closing by a double worm described round its superficies working in an interior screw, contained in a box, opening and closing very considerably by a single revolution of the box round the screw, by the means of a small handspike H.

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* Communicated by the inventor.

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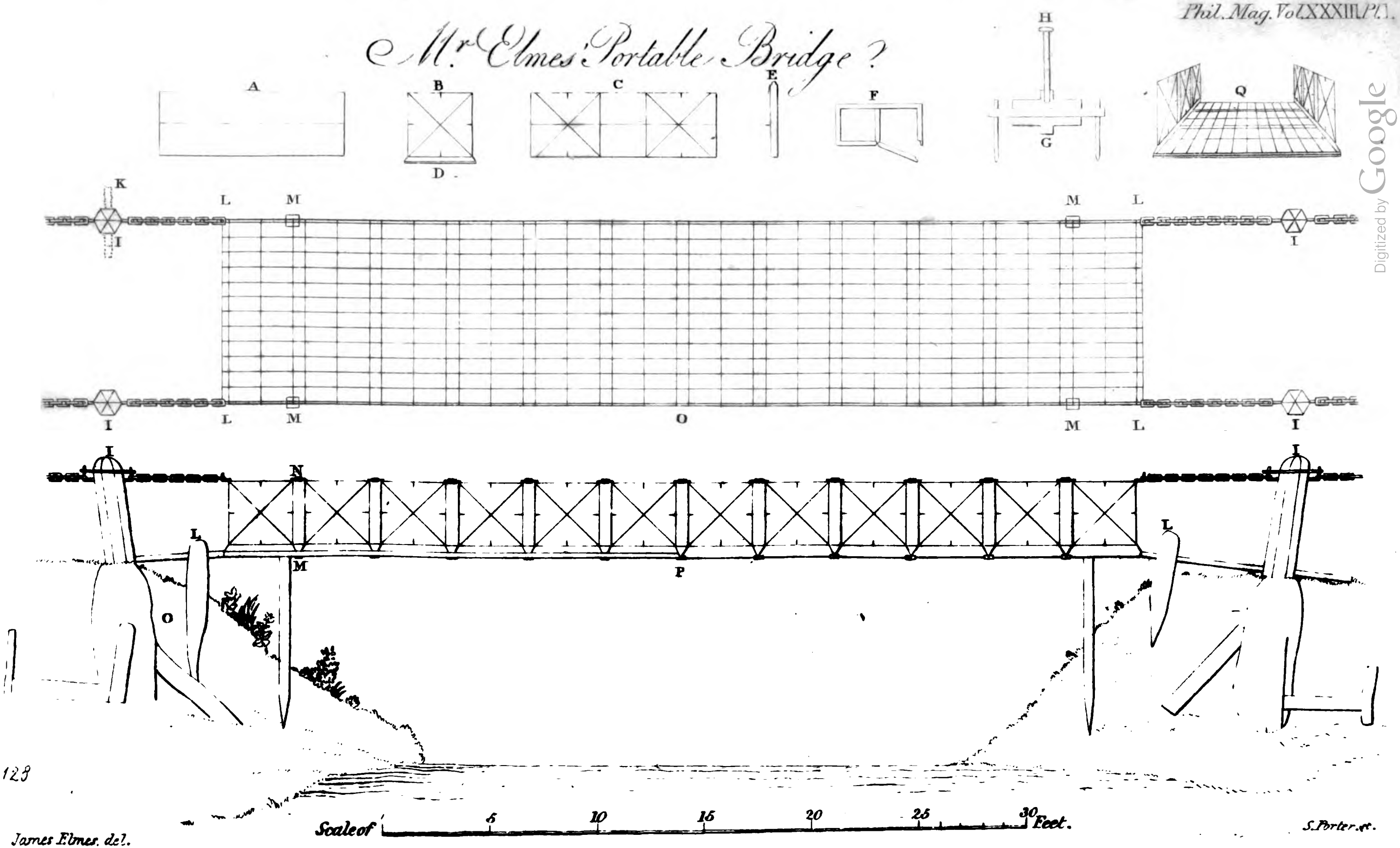
Phil. Mag. Vol. XXXIII. Pl. I.

Mr. Elmes's Portable Bridge.



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James Elmes. del.

S. Porter sc.

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As many of each of the above described parts as are necessary, according to the width of the river or valley intended to be passed, with a sufficient quantity of planking posts, chains, &c., according to the existing circumstances of the case, are the whole of its component parts.

A bridge on this principle for a river, &c., of a considerable width is very portable; for several of the square frames may be packed upon each other in carriages or waggons of the dimensions of eight feet three inches long, three feet four inches wide, and as many feet high as it may be wished to pack a number of frames; two of them rising one foot. The links and staples can be packed in cases, each sort separate.

The method to be used in passing a river with this bridge shall now be described. (A valley is passed in the same manner; but there being no water to pass, the bridge will be easier supported from the under side.) First, two sufficient holes are to be dug on one side the river, at the distance apart of the width of the bridge, which in this plan is nine feet, and the posts I, are first to be prepared with prongs, &c., as in the dotted figure K: next the four smaller ones L, properly secured and well rammed. Then taking any one of the pieces C, fix it on the posts I and L as drawn, and support it on two well driven piles, if the shore will permit: and hooking on the next piece with one of the links F through the eyes at the bottom of the piece, and one of the staples G, fixed into the holes of the upright piece or parapet, it will there hang. Several more are to be hanged on the same way, leaving under them, where necessary, barges or other craft, moored with supporters under them *pro tempore*. When completed to the opposite shore, the same process of fixing the posts, &c., is to be repeated, and when fastened to these posts, the under supports may be taken away, and the whole left suspending by itself. Nothing now remains but for the superintendant of the work to screw the staples, by the handspike H, till the bridge rises by a small curvature by opening the interstices N at the top. It is not required to rise more than a small degree above level, only just enough to stiffen the whole, and cause it

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it to lie like a stiff plank, and rather to occasion a thrust outwards than otherwise, which when the weight has brought down may be again raised by the same operation. The planks are now to be laid on to meet at the intervals as in the ichnographic plan O, of which P is the elevation complete. Q is a perspective view of three joints looking along the bridge with the planks, &c., drawn faintly.

III. *Analysis of some Iron Ores in Burgundy and Franche-Comté; to which is added an Examination of the Pig Iron, Bar Iron, and Scoriæ, produced from them. By M.* VAUQUELIN [*].

IN the year 1805, M. Vauquelin having visited various iron works in Burgundy, collected specimens of ores, pig iron, bar iron, scoriæ, and fluxes; intending to subject them to chemical analyses, to ascertain whether it might be possible to learn, from a comparison of their composition, what takes place in the processes to which iron ores and cast iron are subjected. The following are the principal results of this able chemist's labours, and the particulars of some of the processes he employed to obtain them.

I. *Chemical Examination of some Fluor Spars.*

The spar used as a flux at the mine of Drambon, in the department of Côte-d'Or, is yellowish white, and tolerably hard. It dissolves with effervescence in nitric acid, and leaves a yellowish residuum, amounting to about a fifth of its weight, which is composed chiefly of fine sand, with a minute quantity of alumine and iron. The solution, which is colourless, gives with ammonia a light, flocculent, semi-transparent, yellowish-white precipitate, in which was detected iron, a little alumine, and phosphate of lime. It likewise exhibited some traces of silex.

The spar of Pesme is compact, of a grayish white, and dissolves in nitric acid, leaving a residuum of about a twentieth of its weight. A little iron, alumine, and phosphate of lime, were found in the solution.

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[*] From *Journal des Mines*, No. 119—being an abridgment of a paper given in the Memoirs of the National Institute.

From these two analyses it appears, that the fluors analysed consist almost wholly of calcareous matter, but that of Pesme is the most pure. They show at the same time, that the stones examined contain a small quantity of phosphate of lime, which certainly does not amount to a five-hundredth part.

II. *Analysis of the Scoriæ of the Iron Works at Drambon.*

M. Vauquelin began with these scoriæ, rather than with the ores and smeltings, because these scoriæ include more foreign matters in a smaller bulk.

They have a shining blackish colour, nearly resembling some oxides of manganese. Their weight indicates that a considerable quantity of metallic matter is left in them. Some parts exhibit blebs of different sizes, others are compact. Their fracture is crystallized, either needly or laminar.

Five grammes (77 grains) of scoriæ, fused twice in succession, with an equal weight of caustic potash, communicated to the alkali a very deep green colour, when the mass had been washed with water.—This green colour is known to be an unequivocal proof of the presence of manganese, and it is the best method we can employ to discover the slightest trace of this metal in any substance.

All the washings of the scoriæ thus treated were put together, and boiled, to separate the manganese. In proportion as this took place, the liquor lost its green colour, and the metal floated in it in the form of brown flocks, which, when collected, washed, and dried, weighed two decig. (three grains) amounting to four per cent.—The alkaline liquor, freed from the manganese and filtered, still retained an orange yellow colour, which led M. Vauquelin to suspect the presence of chrome.

To verify this suspicion, it was necessary, in order to facilitate the operations requisite for detecting the chrome, to separate the alumine and silex, that were in the alkaline lixivium: and to avoid the presence of muriatic acid, which would have thwarted the end he proposed, M. Vauquelin employed very pure nitrate of ammonia, instead of the muriate. Thus he obtained two per cent. (0·3 grains) of a mixture of silex and alumine.

He next saturated the liquor with pure nitric acid, added a little in excess, and boiled it for a quarter of an hour, in order to dissipate entirely the carbonic acid.

To a portion of the liquor thus prepared he added a few drops of the solution of nitrate of mercury at a minimum: but instead of yielding a red colour, as is usual with chrome, they threw down a white precipitate, which at first he took for muriate of mercury, but it afterwards appeared to be phosphate of mercury.—Instructed by this trial, he added to the remainder of the liquor limewater, which, when the acid was saturated, produced a flocculent precipitate. This had a slight tint of yellow, which changed to a green on drying, a circumstance that indicated some foreign matter in the phosphate of lime.

Anxious to discover the cause of this colour, he made the precipitate red-hot in a silver crucible; but the green tint, instead of disappearing, became more intense. He then fused a little with borax by the blowpipe, and the fine emerald green colour which the salt assumed, confirmed his first suspicion of the existence of chrome in the scoriæ from the refining furnace.

The remaining precipitate, being treated with nitric acid, did not entirely dissolve; a portion being left of a very deep green colour, which was nothing but oxide of chrome mixed with a little silex, the particles of which being brought together and hardened by the heat had lost the capacity of being soluble.

The solution was colourless; and oxalate of ammonia threw down from it a granulous precipitate, which, when washed and dried, weighed two decig. (three grains), and was true oxalate of lime.

The liquor from which the oxalate of lime was thus precipitated, being evaporated to dryness, and the residuum calcined, yielded an acid, which had all the properties of the phosphoric.

The first liquor, to which the limewater had been added

to

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to precipitate the phosphoric acid, being mixed with nitrate of mercury recently prepared, a brown yellow precipitate was formed, which assumed a green tinge by drying in the air. This precipitate fused with borax gave it a very fine green colour, which proved it to be a chromate of mercury with excess of oxide.

The presence both of chrome and phosphoric acid in the scoriæ from the refining furnace was thus demonstrated. These matters, as well as those that will be mentioned below, existed in the pig iron, and previously in the ore, for nothing was added during the processes of working them, from which these could have been produced.

After chrome, phosphoric acid, manganese, and a portion of the silex and alumine, had been separated, M. Vauquelin dissolved in muriatic acid the ferruginous part, which had then a yellowish red colour. He observed, that, though the alkali had taken from it a great deal of oxide of manganese, a perceptible portion of oxygenized muriatic acid was produced, as the dissolution went on.

A white powder remained at the bottom of the liquor, which, when washed and dried, weighed 88 cent. (13·6 gr.), or about a fifth of the weight of the scoriæ. During the evaporation of the liquor, which was carried to dryness, a portion of the same substance was precipitated, which was freed by means of muriatic acid from a little iron that fell down with it. This contained some traces of chrome, for it communicated to borax a decidedly green colour. It was silex.

M. Vauquelin precipitated the iron from its solution by ammonia, and added to the filtered solution oxalate of ammonia, which formed in it a pretty copious precipitate of oxalate of lime.

The iron, still moist and in an attenuated state, was treated with acetous acid, the mixture evaporated to dryness, and the residuum redissolved in water. In the clear and colourless liquor was detected by different means the presence of oxide of manganese and of alumine, which had escaped the action of the alkali in the first operation, and of a pretty large quantity of lime, which the volatile alkali had precipitated with the help of the oxide of iron.

From these experiments, and the results they furnished, it is evident, that the scoriæ of the refining furnace, on which they were made, are formed of, 1st, a large quantity of iron oxided at a minimum; 2d, oxide of manganese; 3d, phosphate of iron; 4th, chrome, probably in the state of oxide; 5th, silex; 6th, alumine; 7th, lime, part of which is perhaps combined with phosphoric acid.

A doubt can hardly be entertained, that all these matters were contained, at least in part, in the pig iron that furnished the scoriæ: the charcoal might have imparted to them at most some lime, silex, and manganese; but the analysis of the ores, and of the pig iron itself, will soon teach us what we ought to think on this point.

III. *Examination of the Bog Ores.*

The ores subjected to analysis by M. Vauquelin were, 1st, those employed at the forge of Drambon. These are in spherical nodules of different sizes. Some irregular fragments of limestone are observed among them. 2d, those of Chamfont and Grosbois. These much resemble the former. Those of Grosbois contain a pretty large quantity of limestone. 3d, that of Chatillon-sur-Seine. This is of an ochrey yellow colour, in grains as small as millet-seed; no limestone is seen among it, but it contains a pretty large quantity of clay.

M. Vauquelin gives at large his analysis of the ore of Drambon, observing, that the other ores include the same principles, though in different proportions; at the same time the quantities he has assigned to its different component parts he gives only as approximations.

Ten grammes (154·5 grains) of the ore of Drambon, treated with caustic potash, assumed a very intense green colour, that communicated itself to the water in which it was lixiviated. The ore, on being subjected to the same operation a second time, produced a similar effect, but less striking.

The liquors were boiled, and three decig. (4·6 grains) of manganese fell down, containing a little silex, and a minute portion of iron.

The solution retained a slight yellow colour, as in that from the scoriæ. M. Vauquelin, supposing this colour to be produced by the same substance, saturated it with nitric acid. With this liquor he mixed a solution of nitrate of mercury made without heat; when it became colourless, and a white precipitate fell down, which did not give any tinge to glass of borax.

As the liquor contained an excess of acid, it was suspected, that, if any chromate of mercury had been formed, it was held in solution. Accordingly a few drops of a solution of pure potash were added, and a brown red precipitate was obtained, which, being fused with borax, gave in a fine emerald green. This indicated, that it was chromate of mercury, perhaps with a little phosphate of the same metal.

The liquor being still acid, and retaining some mercury in solution, M. Vauquelin imagined it still contained chrome. He therefore added a few drops of nitrate of silver, in hopes of obtaining a crimson red precipitate; but what fell down was of an orange yellow, and did not give a green colour to borax. It was phosphate of silver. Potash added to the remaining liquor produced a very bulky, flocculent, lemon-coloured precipitate. This acquired a green hue as it dried, and was chromate of mercury, containing silver, with a small quantity of alumine and silex.

The mercury was separated from the silver in a gentle heat by means of muriatic acid, diluted with two parts of water, that it might not dissolve the muriate of silver. At once the precipitate became white, and the acid green. The solution being evaporated to dryness left a blackish matter, which gave a very fine green colour to borax.

Afterward, by employing sulphuric acid, and precipitating by limewater, M. Vauquelin obtained 1·5 per cent. of magnesia. Though this earth was found in the pig iron from each of the five bog ores, he does not venture to assert that it exists in all: but he observes, he has much more reason to think that chrome and phosphoric acid are constantly found in it.

Reflecting that oxide of manganese, chrome, and magnesia, which he had just obtained, were found likewise in aërolites, or meteoric stones, he questioned whether it were not possible for iron ores to have contributed in some way or other to the formation of these stones. This idea led him to examine, whether nickel likewise did not occur in bog ores; but his researches were fruitless.

From what has been said it follows, that the bog ores analysed were composed of, 1st, iron; 2d, manganese; 3d, phosphoric acid; 4th, chrome; 5th, magnesia; 6th, silex; 7th, alumine; and 8th, lime. The chrome, phosphoric acid, and magnesia had not before been noticed in these ores.

IV. *Examination of the Iron that sublimes and collects in the Chimneys of the refining Furnace.*

This iron is found adhering to the sides of the chimneys of the refining furnace in the shape of stalactites, which are sometimes more than a foot long and three or four inches in diameter. They are formed of agglutinated grains, red in their fracture, leaving great intervals between them, and having but a slight action on the magnet.

We shall omit the particulars of M. Vauquelin's analysis, which he concludes with the following words:

" In this sublimed iron, then, there are oxide of manganese, silex, phosphoric acid, and above all a great deal of chrome. These matters therefore have been volatilized by the caloric, either by being dissolved in this fluid, or by yielding to the impulse of the current of air; but in either case they have issued from the pig iron during the process of refining."

V. *Examination of the Pig Iron of Drambon.*

Having found oxide of manganese, chrome, phosphoric acid, and earths, in the scoriæ of the refining furnace, it was natural for M. Vauquelin to infer, that he should find the same substances in the pig iron; since it is this that furnishes these scoriæ, at least for the most part, in the process of refining. This fact was fully confirmed by analysis.

He proceeded thus. Ten grammes (154·5 grains) of gray pig iron of Drambon reduced to filings were dissolved in sulphuric acid diluted with six parts of water. The hydrogen gas evolved during the solution was collected. It had an extremely fetid smell, very much resembling that of phosphuretted hydrogen gas, though it had a certain pungency, which the phosphuretted hydrogen has not. The nature of this gas will be noticed presently.

The residuum was of a very deep black, and diffused an extremely strong smell of phosphorus. It weighed 53 cent. (8·2 grains), or a little more than a twentieth of the iron employed. The upper part of the bottle in which the solution was made, and the tube through which the hydrogen had passed, being so greasy that water would not adhere to them, M. Vauquelin suspected that oil had been formed; a fact fist announced by M. Proust a few years ago on a similar occasion, and which M. Vauquelin adds he had himself observed before that, when dissolving certain kinds of tin.—To know whether any of this oil remained in the residuum of the pig iron dissolved in the sulphuric acid, he boiled it with highly dephlegmated alcohol, and filtered the liquor hot.

On the addition of water this alcohol became milky; and being exposed to a gentle heat, drops of oil separated from it as the alcohol evaporated. This oil was clear and transparent; it had a slight yellow tinge; its taste was hot and a little pungent. It appeared to be of a middle kind between the volatile and fat oils.

When the oil it contained was separated from the residuum of the pig iron, this residuum was deflagrated in a silver crucible with a little very pure nitrate of potash, the matter was washed with distilled water, and a light yellow liquor was obtained. This was mixed with a solution of the nitrate of ammonia, to precipitate the silex and alumine supposed to be contained in it; and a small quantity of these was separated. Limewater added to the filtered liquor formed in it a copious precipitate, which had all the characters of phosphate of lime.

To ascertain whether chrome was contained in this liquor, it was first boiled to volatilize the ammonia, and a few drops of nitrate of mercury were added, which was precipitated of a brown yellow, owing to a little lime remaining. This precipitate, however, gave a green colour to borax, which proves that it contained chrome.

The lixivium from the residuum of the solution calcined with nitrate of potash then contained phosphoric acid, chrome, and silex mixed with a little alumine. There was likewise in it a minute portion of manganese.—The residuum, when thus treated and lixiviated, was in the form of a reddish powder, which was dissolved for the greater part by muriatic acid. There remained, however, a small quantity of grayish matter, which was silex mingled with chrome, for it gave a very decided green colour to borax.—The muriatic solution contained a large portion of iron. It assumed the consistence of a jelly on evaporation, which proves that it contained silex. It is probable that a little chrome and manganese were also concealed in it.

It appears then that this pig iron, besides carburet of iron, contains phosphuret of iron, manganese, chrome, silex, and alumine. Next to the iron and carbon, it appeared to M. Vauquelin that the phosphorus was most abundant. It is then in the residuums of the solutions of pig and bar iron that we must henceforward look for phosphorus, rather than in the solutions themselves, as has hitherto been done. Probably the neglecting to examine these residuums with sufficient attention is the reason of our remaining so ignorant of the causes of the bad quality of iron.

M. Vauquelin admits that there is likewise a small quantity of phosphorus converted into acid, and dissolved in the liquor, probably in the state of phosphate of iron, by means of the sulphuric acid. It appears to him, that, when the sulphuric acid is less diluted with water, a larger quantity of phosphorus dissolves in the liquor. To separate this phosphate of iron, he dilutes the solution with seven or eight

parts

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parts of water, and mixes with it carbonate of potash, till almost the whole of the acid is saturated. A white precipitate is formed, more or less copious according to the kind of iron employed; and at the expiration of a few days it grows yellowish. This precipitate, washed and dried, he treats with potash in a silver crucible at a red heat: he then lixiviates the matter with water, and, after having saturated the liquor with nitric acid, and boiled it to expel the carbonic acid, he adds limewater, which commonly forms a white flocculent precipitate, or semitransparent if phosphoric acid be present. He has likewise found a large quantity of chrome in the precipitate produced by carbonate of potash in the solution of pig iron by sulphuric acid. It follows therefore, that chrome as well as phosphorus is oxygenized and dissolved in sulphuric acid.

The alkaline liquor should be tested with nitrate of ammonia, previously to saturating it, in order to know whether it holds any silex or alumine in solution. If it does, a sufficient quantity should be added to precipitate these earths, after which they must be separated by the filter; as without this precaution they would be precipitated by the lime, and might be mistaken for phosphate of lime. M. Vauquelin has found very evident traces of this salt in the pig iron of the works at Drambon, though he employed sulphuric acid diluted with six parts of water to dissolve it: there was much less, however, than remained in the residuum of the solution. This was the only kind of pig iron he examined, but he conceives it probable that all the irons from bog ores contain the same foreign matters.

VI. *Examination of the Bar Iron of Drambon and Pesmes.*

M. Vauquelin dissolved five grammes (77·2 grains) of cold short iron of Drambon in sulphuric acid diluted with five parts of water. The hydrogen gas evolved during the dissolution had exactly the same smell as that of the gas from the pig iron, but not quite so powerful.—The residuum left by these five grammes was much less copious than that of the pig iron, and appeared likewise not to be of so deep a black. While wet, it emitted a very strong fetid smell,

analogous

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analogous to that of hydrogen gas. It weighed 15 cent, (2·3 grains), amounting to three per cent. The solution of the iron had the same smell, which was not dissipated but by evaporation.

A few particles of this residuum, thrown on a burning coal, emitted a white vapour, with a smell resembling that of arsenic and phosphorus. Heated red-hot in a silver crucible, it burned with flame, and left behind a yellowish powder. This was mixed with a little caustic potash, calcined, and lixiviated. The liquor being filtered, saturated with nitric acid, and subjected for a few minutes to heat, limewater was added, which threw down a white flocculent precipitate, consisting chiefly of phosphate of lime, but with a minute portion of silex, and perhaps of alumine.

It is certain from these experiments, which M. Vauquelin repeated several times, that the iron of Drambon, though it is considered as of pretty good quality, contains very perceptible traces of phosphorus. He likewise found some slight traces of it in the solution by sulphuric acid.

The iron of Pesmes afforded nearly the same results. The residuum, however, was less by one half, amounting only to 1½ per cent.; and it contained less phosphorus. This iron is very tough, and is reckoned one of the best in Franche-Comté.

VII. *Of the Hydrogen Gas.*

Various experiments, which M. Vauquelin made by the help of oxygenated muriatic acid on the hydrogen gas evolved from the pig and bar iron, led him to conclude that phosphorus is the chief cause of its fetid smell.

VIII. *Recapitulation and Inferences.*

From the experiments I have related, says M. Vauquelin, it follows;

1. That the five sorts of bog ore I analysed are composed of the same principles, which, beside iron, are silex, alumine, lime, oxide of manganese, phosphoric acid, magnesia, and chromic acid.

2. That the five sorts of ore having been taken without

selection from places tolerably distant from each other, it is probable that all ores of the same kind contain the same substances.

3. That these ores want only nickel, to contain the same substances as the stones that have fallen from the atmosphere.

4. That part of these substances remains in the bar iron, and probably in larger quantity in pig iron, which may be the cause of its greater hardness and brittleness.

5. That the greater part of these substances is separated during the refining of the pig iron, when this operation is well executed; since they are found in the scoriæ, and in the sublimed iron that adheres to the insides of the chimneys of the refining furnaces.

6. That traces of them, however, are found in bar iron of good quality; and that probably chrome, phosphorus, and manganese are the chief causes that render iron hot short or cold short.

7. That the process of refining merits the greatest attention from iron-masters, since it appears that the good quality of iron depends on its skilful execution.

8. That the presence of phosphorus and of chrome is to be sought for not in the solutions of pig and bar iron alone, but also in the residuums of their solutions.

9. That by the union of hydrogen and carbon during the dissolution of iron, and particularly of gray cast iron, an oil is formed, which, in conjunction with a small quantity of phosphorus, communicates a fetid smell to the hydrogen gas that dissolves them.

10. That it is to these two substances the hydrogen gas owes its properties of burning with a blue flame, and being heavier than when pure.

11. Lastly, That the oil and the phosphorus are separated from the hydrogen gas by oxygenized muriatic acid, which destroys them.

IV. *On Hydrophobia.*

To Mr. Tilloch.

SIR,

The following Paper, with some other MSS., lately fell into my hands. The Paper now sent appears to have been written several years ago. If you think it worthy of insertion in your valuable Journal, it is very much at your service.

I am your very obedient servant,

Greville street,
Jan. 10, 1809.

JOHN TAUNTON.

Rabies canina, or, as it is more commonly called, Hydrophobia, the subject of the following dissertation, is a disease as little understood, yet as serious in its consequences, and dreadful in its effects, as any with which the human body is affected.

It may be defined a painful and difficult state of deglutition, attended with great anxiety and horror of countenance, with occasional convulsive paroxysms; and these the consequence of the bite of a mad animal.

History of the Disease.

The symptoms take place at very irregular and uncertain intervals of time after the bite, having been known to occur as early as the third week, and as late as nine or twelve months; but for the most part the commencement of the disease may be placed at four or six weeks from the time of the accident. In most cases, the first symptom is a painful and uneasy sensation in the part where the bite was inflicted; but this is not to be considered as a constant or invariable occurrence.

Among the earliest appearances are to be ranked languor, depression of spirits, timidity, disturbed sleep, frightful dreams, sighing, and loss of appetite; sometimes with nausea, weight at the stomach, and rigor.

In a short time the unhappy object becomes extremely sensible to all external impressions, the sense of touch, of hearing and seeing, being more or less affected in different cases. Upon attempting to swallow the smallest quantity either of solids or fluids, but especially the latter, although frequently

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frequently excited to it by thirst, the greatest agitation and horror are produced, with an apparently strong convulsive affection of the pharynx and œsophagus, difficulty of breathing, tremor, great anxiety and impatience, and remarkable quickness of circulation.

The patient is not so much agitated by the sight of solids or fluids in the early state of the disease, as is commonly imagined; watery liquors being sometimes carried to the mouth with fortitude and composure; but, immediately on touching the lips, are rejected with a violent and frightful agitation, the mind being at that time more particularly conscious of the inability of swallowing.

Solids are, notwithstanding, in some cases, got down, even in the advanced stage of the disease, but never without pain and agitation, being thrust into the mouth in a peculiar hurried and greedy manner, and invariably exciting or increasing the convulsions, which constitute so formidable a part of the disease.

After these symptoms have continued 12, 18, or 24 hours (the progress being somewhat different in different cases), the disorder puts on the most distressing and melancholy appearance; the convulsive attacks, which were before excited chiefly by the attempt to swallow, now occur spontaneously every 10 or 15 minutes, the whole body being so violently agitated as to require several assistants to support the patient. The countenance is wild, the eyes red and staring, and large drops of sweat pour from the head and face.

In the intervals of these paroxysms, the miserable sufferer becomes somewhat composed, complains of an uneasy sensation across the breast, and also in the throat, often ascribing it to wind, and wondering that he is not able to discharge it, so as to obtain relief.

The secretion of saliva is now much increased, but of such a thick viscid quality, that the patient is obliged to exert considerable force to discharge it from his mouth. The manner of doing this, and the frequency with which it is repeated, joined to the peculiar anxious state of the countenance already mentioned, so strongly characterize the disease, that the most superficial observer, having seen one

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case of it, can scarcely afterwards be at a loss to distinguish another. As the disease draws towards a conclusion, the intellectual faculties, which had before remained wonderfully perfect, give way, the patient being affected with delirium of the fiercest and most unmanageable kind, especially during the paroxysms of convulsion, which become so frequent as scarcely to have any interval.

The strength is at length exhausted; the pulse is extremely small, weak, quick, and intermitting; cold clammy sweats supervene; the countenance is somewhat livid and frightfully distorted; and in this state a general return of convulsion puts an end to one of the most melancholy and affecting scenes that the human mind can well form an idea of. This fatal termination happens most commonly about the end of the third day from the first attack, though it has sometimes occurred as early as the second, and at other times as late as the fourth day.

No anatomical examination that has hitherto been made on this subject, seems to have thrown any light upon it. Different parts of the fauces, pharynx, and œsophagus have been frequently found slightly inflamed, probably owing to the exertions to which these parts are subjected in the course of the disease. The lungs have also been generally found distended with blood, and in some cases the vessels of the brain likewise; both of which may be supposed to depend on the irregular action of the heart. The only disease with which this is likely to be confounded is tetanus; the painful and difficult deglutition, with the convulsive paroxysms, being common to both; but the continued stiffness of the jaws, or the spasm of the muscles by which the jaws are kept fixed, which is essential to tetanus, will at once lead to a satisfactory distinction, independent of the circumstances of infection, which we always annex to hydrophobia.

With regard to the theory of this disease, there are several important questions which naturally present themselves for consideration, each requiring a separate and perhaps extensive discussion, viz., To what is the origin of the infection to be ascribed? To what animals is it confined? Is any country exempted from it, and in what climates is it most frequent? Is the infection confined to the saliva? Is absorption necessary to the production of the disease? At how late a period is the destruction of the part on which the bite was inflicted, effectual in preventing the disorder? Are the symptoms accompanied by a state of increased excitement, or by debility?

The limits of this paper will not permit me to enlarge on these interesting topics; but we may shortly observe, that, whatever may be the origin of the infection, it does not appear to be confined to any particular class of animals, or any particular country or climate; that, with regard to absorption as necessary to the production of the disease, it is difficult to form a decided opinion; for while on the one hand we have the analogy of other poisons, as that of lues venerea, in favour of absorption, we must observe on the other, that this disease bears a great resemblance to tetanus, where there is not the most distant suspicion of any thing absorbed to account for the mischief; and further, that the lymphatic glands in the course of absorption have never, as far as I know, been found diseased.

Could this point be settled, we should have less difficulty in determining the next, namely, the period at which the destruction of the part bitten would be effectual in preventing the occurrence of the disease; for, were it clearly shown to be produced by absorption, we should be inclined to think that the operation would be successful any time before the commencement of the pain in the part mentioned in the history.

As to the question whether the hydrophobia be a disorder accompanied by increased excitement or debility, it is necessary to say, that, by increased excitement, I mean not only a greater frequency of action in the heart and arteries, but of strength likewise. In some cases of this disease, the symptoms seem to have indicated such a condition; as, for example, the rapid and apparently strong state of circulation, and the fierce and unmanageable delirium. Bleeding has seemed to be strongly indicated from these circumstances, and has accordingly had a full and fair trial; but the effects have by no means tended to confirm the idea on which the practice was founded. The delirium and frequency of circulation have rather, on the contrary, been increased by it. It is not unlikely, when we take a view of the action of the other poisons, that there may be a difference in different cases; thus, the small-pox is sometimes attended with strong inflammatory symptoms; at other times with great debility; and this difference not depending on any variety in the poison itself, but on the state of the constitution and other accidental circumstances. I am not able to determine how far the analogy will apply to hydrophobia. That it is sometimes connected with debility cannot be doubted, having occurred in delicate children, in whom the pulse has been weak throughout the whole of the disease. This weakness, too, must necessarily be increased by the unhappy state of deglutition, which precludes all nourishment by the mouth. There are several other circumstances also, which are favourable to the idea of debility as connected with this complaint: thus, it is well known that some of the most violent convulsive disorders are attended with great debility, and the only method of removing them effectually, is by strengthening the constitution. Again, a fierce and very unmanageable state of delirium occurs in some cases of low fever, where every other symptom points out weakness, and the free use of wine at such times has produced the happiest effects. Although then it is far from my intention to propose debility as explanatory of the symptoms of hydrophobia, yet it appears to be that condition of the body which most generally accompanies it, and which should not be lost sight of in the treatment.

Method of Treatment.

I have no hesitation in affirming, that there is no well-marked case of this disorder in which a cure has been obtained after the symptoms have made their appearance; and even with regard to the prevention, that there is no method of treatment (that of removing the part excepted) which can in any degree be depended on.

The disorder has repeatedly occurred after the fullest trial of the Ormskirk and other boasted specifics. And no favourable conclusion can be drawn from those instances in which patients having taken such remedies have escaped. First, because many bites are inflicted by animals which are not diseased, but only supposed to be so. Secondly, If the animal be decidedly mad, all of those on whom the bite is inflicted are not the subjects of the disease, some of them escaping independently of any medicine.

After the symptoms have made their appearance, there are some remedies which appear to have had so full a trial, that their exhibition should be totally laid aside in future. Of these are the Ormskirk medicine, musk, mercurials, bleeding, warm bath, and opium; and therefore, in conducting the treatment hereafter, I would propose in the first place, that we should seek for a specific among those articles of the materia medica which are known to exert strong effects upon the body. Among the metallic preparations, I would more particularly recommend a trial of those of lead, copper, zinc, and lastly of arsenic. Among the vegetables, tobacco, cicuta, aconite, henbane, &c. Several remedies of this description may be administered at the same time.

But while we are endeavouring in this way to find out a specific for the poison of hydrophobia, I would not neglect other objects, which appear to be of consequence, and which do not interfere with it. Thus, I should endeavour to administer frequent clysters composed of broth, milk, and other nutritious articles. Various antispasmodics may be combined and employed in the same form, for it is in vain to expect that the patient can swallow so frequently as would be necessary to the fair trial of such remedies. Camphor, asafœtida, castor, æther, &c., may all be combined in the form of clyster, and injected every second or third hour. When these different plans have been tried, if the disease should still baffle our endeavours, let us not continue tame witnesses of so melancholy a spectacle, but proceed to methods which no other situation could justify—I mean that of injecting into the blood vessels various active remedies, having previously tried upon animals (as some kind of guide) in what quantities they can be received into circulation without fatal effects. If these should be found unsuccessful, to expose the patient for a certain length of time to one or other of the mephitic gases.

V. *On Deal Pendulum Rods.*

Lynn, Dec. 17th, 1808.

To Mr. Tilloch.

SIR,

It has been frequently observed, that clocks with wooden pendulum rods vary considerably in their rates of going, at different seasons of the year; but the cause of this irregularity still remains in some obscurity, for want of a greater number of observations.

Mr. Ludlam says, "That such a pendulum rather loses in cold and gains in warmer weather *." Mr. Wollaston had a clock with a pendulum rod of deal, and he says, "It appears as if the clock gained in warm and lost in cooler weather: but this is not clear. It began to gain before the weather grew warm. Whether this be owing to damp, or any other causes, longer experience and abler observers may discover †."

My clock, of the rate of which the following table contains a short abstract from 1798 to 1807, has a deal pendulum rod, a dead escapement, and goes when winding up. The daily rate was ascertained by a transit instrument which stands in the same room with the clock, and the observations were taken at all convenient opportunities, as an exact rate cannot be found from observations taken only once in a fortnight or three weeks.

It appears from this table, that pendulums with wooden rods gain most in the driest, and lose most in the dampest weather. I could never discover that heat or cold had any effect upon my clock, further than that it went very regularly during hard frosty weather, which I suppose was owing to the moisture in the pendulum being frozen. And it also appears, that moisture does not affect the wood so much as the contrary extreme, but the greatest rate of gaining was only of short duration: hence it may be supposed, that the wood imbibed moisture rapidly as soon as the dry season was over.

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* Ludlam's Observations, p. 40.

† Phil. Trans. abridged, No. 50, p. 216.

I am, sir, your humble servant,

Ez. Walker.

Table of the greatest Variations in the daily Rates of three Clocks with Deal Pendulum Rods.

	Time of the year when the greatest loss and gain in the daily rate of the clock took place.	Greatest variation in the daily rate of the clock in twelve months.	Greatest annual variation, in the rate of the clock.
1798.	Between Feb. 4th and the 6th,	− 0·73″	4·83″
	Sep. 14th —— 15th,	+ 4·10	
1799.	Feb. 24th —— 25th,	− 1·14	3·67
	July 25th —— 26th,	+ 2·53	
1800.	Feb. 4th —— 5th,	− 2·4	6·4
	Aug. 18th —— 27th,	+ 4·0	
1801.	Jan. 7th —— 8th,	− 2·59	6·1
	July 11th —— 14th,	+ 3·51	
1802.	March 7th —— 9th,	− 2·48	5·64
	Oct. 11th —— 13th,	+ 3·16	
1803.	Feb. 18th —— 23d,	− 2·33	5·81
	Sep. 25th —— 29th,	+ 3·48	
1804.	*Another pend. with a wooden rod put to the same clock.*		
1806.	Between Jan. 24th and the 25th,	− 2·91	5·39
	Do. June 20th —— 22d,	+ 2·48	
1806.	Do. Dec. 27 & Jan. 1. 1807,	− 1·35	4·22
1807.	Do. Aug. 7th and the 9th,	+ 2·87	
	The Rev. Francis Wollaston's Clock.*		
1770.	Between Dec. 22d and the 30th	− 1·9	4·3
1771.	Do. June 1st —— 18th	+ 2·4	
	From Ludlam's Observations, p. 44.		
1767.	Between Oct. 30th & Nov. 13th,	− 2·2	5·4
1768.	Do. May 18th and the 31st,	+ 3·2	

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* Philosophical Transactions abridged, No. 50, p. 216.

VI. *An Account of a Method of hastening the Maturation of Grapes.* *By* JOHN WILLIAMS, *Esq., in a Letter to the Rt. Hon. Sir* JOSEPH BANKS, *Bart. K.B. P.R.S.**

SIR,

IT is a fact well known to gardeners, that *vines*, when exposed in this climate to the open air, although trained to walls with southern aspects, and having every advantage of judicious culture, yet in the ordinary course of our seasons ripen their fruit with difficulty. This remark, however, though true in general, admits of some exceptions; for I have occasionally seen trees of the common *white muskadine*, and *black cluster grapes*, that have matured their fruit very well, and earlier by a fortnight or three weeks than others of the same kinds, and apparently possessing similar advantages of soil and aspect.

The *vines* that ripened the fruit thus early, I have generally remarked, were old trees having trunks eight or ten feet high, before their bearing branches commenced. It occurred to me, that this disposition to ripen early, might be occasioned by the dryness and rigidity of the vessels of the old trunk obstructing the circulation of that portion of the sap which is supposed to descend from the leaf. And to prove whether or not my conjectures were correct, I made incisions through the bark on the trunks of several vines growing in my garden, removing a circle of bark from each, and thus leaving the naked alburnum above an inch in width completely exposed; this was done in the months of *June* and *July*. The following autumn the fruit growing on these trees came to great perfection, having ripened from a fortnight to three weeks earlier than usual: but in the succeeding spring the vines did not shoot with their accustomed vigour, and I found that I had injured them by exposing the alburnum unnecessarily.

Last summer these experiments were repeated; at the end of *July* and beginning of *August*, I took annular excisions of bark from the trunks of several of my vines, and that the exposed alburnum might be again covered with new bark by the end of autumn, the removed circles were made rather less than a quarter of an inch in width. Two vines of the *white Frontiniac*, in similar states of growth, being trained near to each other on a south wall, were selected for trial; one of these was experimented on (if I may use the term), the other was left in its natural state, to form a standard of comparison. When the circle of bark had been removed about a fortnight, the berries on the experimented tree began evidently to swell faster than those on the other, and by the beginning of *September* showed indications of approaching ripeness, while the fruit of the unexperimented tree continued green and small. In the beginning of *October*, the fruit on the tree that had the bark removed from it was quite ripe, the other only just began to show a disposition to ripen, for the bunches were shortly afterwards destroyed by the autumnal frosts. In every case in which circles of bark were removed, I invariably found that the fruit not only ripened earlier, but the berries were considerably larger than usual, and more highly flavoured.

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* From Transactions of the Horticultural Society, vol. i.

The effects thus produced, I can account for only, by adopting Mr. Knight's theory of the downward circulation of the sap, the truth of which these experiments, in my opinion, tend strongly to confirm. I therefore imagine by cutting through the cortex and liber without wounding the alburnum, that the descent of that portion of the sap which has undergone preparation in the leaf is obstructed and confined in the branches situated above the incision; consequently the fruit is better nourished and its maturation hastened. It is certainly a considerable point gained in the culture of the vine, to be able to bring the fruit to perfection, by a process so simple, and so easily performed. But lest there should be any misconception in the foregoing statement, I will briefly describe the exact method to be followed by any person who may be desirous of trying this mode of ripening grapes. The best time for performing the operation on vines growing in the open air, is towards the end of *July*, or beginning of *August*, and it is a material point, not to let the removed circle of bark be too wide: from one to two eighths of an inch will be a space of sufficient width; the exposed alburnum will then be covered again with new bark before the following winter, so that there will be no danger of injuring the future health of the tree.

It is not of much consequence in what part of the tree the incision is made, but in case the trunk is very large, I should then recommend, that the circles be made in the smaller branches.

It is to be observed that all shoots which come out from the root of the vine, or from the front of the trunk situated *below* the incision, must be removed as often as they appear, unless bearing wood is particularly wanted to fill up the lower part of the wall, in which case one or two shoots may be left.

Vines growing in forcing houses are equally improved in point of size and flavour, as well as made to ripen earlier by taking away circles of bark: the time for doing this, is when the fruit is set, and the berries are about the size of small shot. The removed circles may here be made wider than on vines growing in the open air, as the bark is sooner renewed in forcing houses, owing to the warmth and moisture in those places. Half an inch will not be too great a width to take off in a circle from a vigorous growing vine, but I do not recommend the operation to be performed at all in weak trees.

I think that this practice may be extended to other fruits, so as to hasten their maturity, especially *figs*, in which there is a most abundant flow of returning sap; and it demonstrates to us, why old trees are more disposed to bear fruit than young ones. Miller informs us, that the vineyards in *Italy* are thought to improve every year by age, till they are 50 years old. It therefore appears to me, that nature, in the course of time, produces effects similar to what I have above recommended to be done by art. For, as trees become old, the returning vessels do not convey the sap into the roots, with the same facility they did when young: thus, by occasionally removing circles of bark, we only anticipate the

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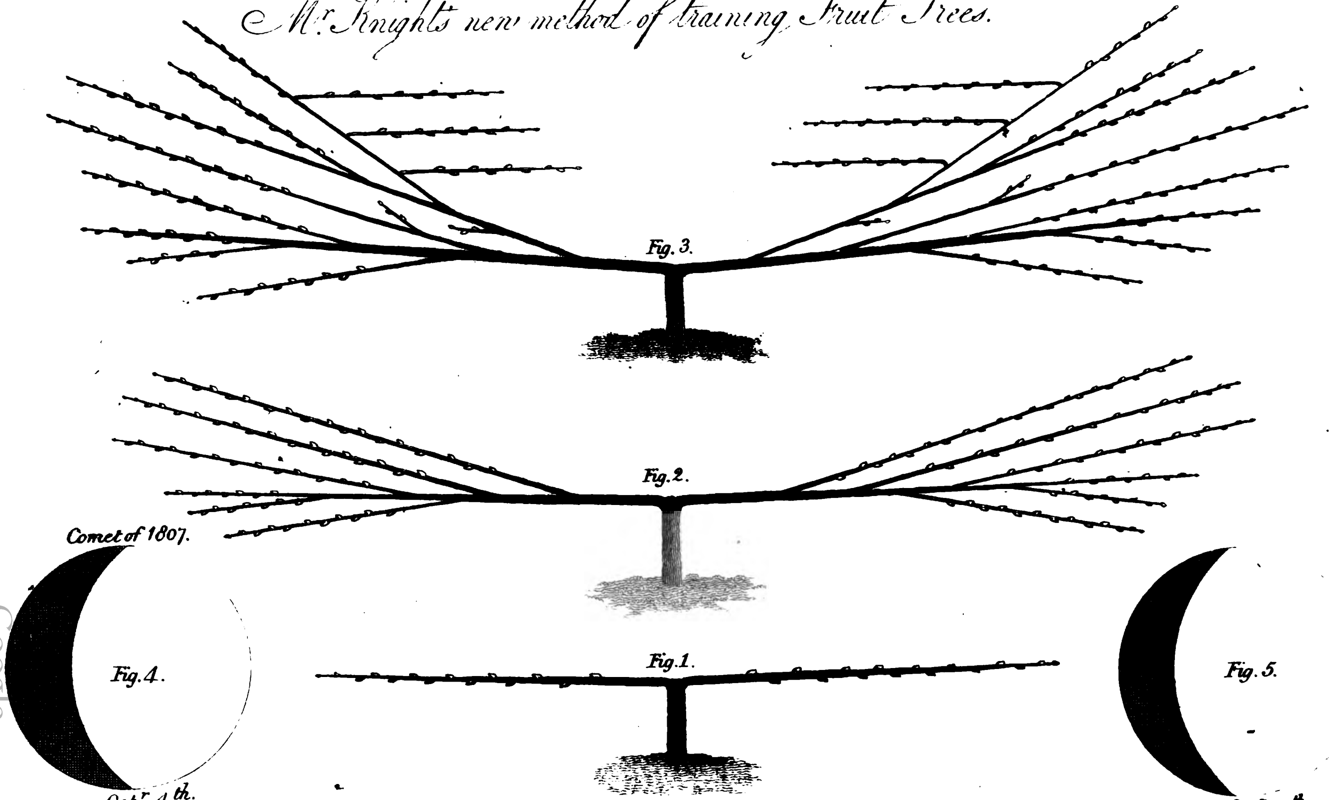
Mr. Knight's new method of training Fruit Trees.



S. Porter, sc.

the process of nature; in both cases a stagnation of the true sap is obtained in the fruiting branches, and the redundant nutriment then passes into the fruit.

I have sometimes found that after the circle of bark has been removed, *a small portion of the inner bark has adhered to the alburnum:* it is of the utmost importance to remove this, though ever so small, otherwise in a very short space of time, the communication is again established with the root, and little or no effect produced. Therefore in about ten days after the first operation has been performed, I generally look at the part from whence the bark was removed, and separate any small portion, which may have escaped the knife the first time.

I am, sir, your obedient servant,

JOHN WILLIAMS.

Pitmaston, Worcestershire,
April 20, 1808.

VII. *On a new Method of training Fruit Trees. By* THOS. ANDREW KNIGHT, *Esq., F.R.S., &c.*[*]

FROM the result of experiments I have made to ascertain the influence of gravitation on the descending sap of trees, and the cause of the descent of the radicle, and ascent of the expanding plumule of germinating seeds[†], I have been induced to believe that none of the forms, in which fruit trees are generally trained, are those best calculated to promote an equal distribution of the circulating fluids; by which alone permanent health and vigour, and power to afford a succession of abundant crops, can be given. I have therefore been led to try a method of training which is, I believe, different from any that has been practised; and as the success of this method has fully answered every expectation I had formed, I have thought a concise account of it might not be unacceptable to the Horticultural Society. I confine my account to the peach tree, though, with a little variation, the method of training and pruning, that I recommend, is applicable, even with superior advantages, to the cherry, plum, and pear tree; and I must observe, that when trees are by any means deprived of the motion, which their branches naturally receive from winds, the forms in which they are trained, operate more powerfully on their permanent health and vigour, than is generally imagined.

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[*] From Transactions of the Horticultural Society, vol. i.

[†] Philosophical Transactions, 1806 and 1807.

My peach trees, which were plants of one year old only, were headed down, as usual, early in the spring, and two shoots only were trained from each stem in opposite directions, and in an elevation of about five degrees; and when the two shoots did not grow with equal luxuriance, I depressed the strongest, or gave a greater elevation to the weakest, by which means both were made to acquire and to preserve an equal degree of vigour. These shoots, receiving the whole sap of the plants, grew with much luxuriance, and in the course of the summer each attained about the length of four feet. Many lateral shoots were of course emitted from the young luxuriant branches; but these were pinched off at the first or second leaf; and were in the succeeding winter wholly destroyed; when the plants, after being pruned, appeared as represented in Plate II. Fig. 1. This form, I shall here observe, might with much advantage be given to trees whilst in the nursery; and perhaps it is the only form which can be given, without subsequent injury to the tree: it is also a form that can be given, with very little trouble or expense to the nurseryman.

In the succeeding season as many branches were suffered to spring from each plant as could be trained conveniently, without shading each other; and by selecting the *strongest* and *earliest* buds towards the points of the year old branches, and the *weakest* and *latest* near their bases, I was enabled to give to each annual shoot nearly an equal degree of vigour; and the plants appeared in the autumn of the second year nearly as represented in Fig. 2. The experienced gardener will here observe, that I exposed a greater surface of leaf to the light, without placing any of the leaves so as to shade others, than can probably be done in any other mode of training; and in consequence of this arrangement, the growth of the trees was so great, that at two years old some of them were fifteen feet wide; and the young wood in every part acquired the most perfect maturity. In the winter, the shoots of the last season were alternately shortened, and left their whole length, and they were then prepared to afford a most abundant and regular blossom in the succeeding spring.

In the autumn of the third year the trees were nearly as represented in Fig. 3, the central part of each being formed of very fine bearing wood; and the size and general health of the trees afford evidence of a more regular distribution of the sap, than I have witnessed in any other mode of training.

In the preceding method of treating peach trees very little use was made of the knife during winter; and I must remark that the necessity of winter pruning should generally be avoided as much as possible; for by laying in a much larger quantity of wood in the summer and autumn than can be wanted in the succeeding year, the gardener gains no other advantage than that of having a "great choice of fine bearing wood to fill his walls," and I do not see any advantage in his having much more than he wants; on the contrary, the health of the tree always suffers by too much use of the knife through successive seasons.

To enter into the detail of pruning, in the manner in which I think it might be done with most advantage, would of necessity lead me much beyond the intended limits of my present communication; but I shall take this opportunity of offering a few observations on the proper treatment of luxuriant shoots of the peach tree, the origin and office of which, as well as the right mode of pruning them, are not at all understood either by the writers on gardening of this country, or the Continent.

I have shown in the Philosophical Transactions of 1805, that the alburnum or sap wood of oak trees loses a considerable part of its weight during the period in which its leaves are formed in the spring; and that any portion of the alburnum affords less extractive matter after the leaves have been formed than previously. I have also shown that the aqueous fluid which ascends in the spring in the birch and

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sycamore becomes specifically heavier as it ascends towards the buds; which, I think, affords sufficient evidence that the alburnum of trees becomes during winter a reservoir of the sap or blood of the tree, as the bulb of the hyacinth, tulip, and the tuber of the potatoe, certainly do of the sap or blood of those plants. Now a wall-tree, from the advantageous position of its leaves relative to the light, probably generates much more sap, comparatively with the number of its buds, than a standard tree of the same size; and when it attempts to employ its reserved sap in the spring, the gardener is compelled to destroy (and frequently does so too soon and too abruptly) a very large portion of the small succulent shoots emitted, and the aphis too often prevents the growth of those which remain. The sap in consequence stagnates, and appears often to choke the passages through the small branches; which in consequence become incurably unhealthy, and stunted in their growth: and nature then finds means of employing the accumulated sap, which, if retained, would generate the morbid exudation, gum, in the production of luxuriant shoots. These shoots, our gardeners, from Langley to Forsyth, have directed to be shortened in summer, or cut out in the succeeding spring; but I have found great advantages in leaving them wholly unshortened; when they have uniformly produced the finest possible bearing wood for the succeeding year; and so far is this practice from having a tendency to render naked the lower or internal parts of the tree, whence those branches spring, that the strongest shoots they afford invariably issue from the buds near their bases. I have also found that the laterals that spring from these luxuriant shoots, if stopped at the first leaf, often afford very strong blossoms and fine fruit in the succeeding season. Whenever therefore space can be found to train in a luxuriant shoot, I think it should rarely or never be either cut out, or shortened: it should, however, never be trained perpendicularly, where that can be avoided.

VIII. *Proposed Improvement of the Hygrometer.*
By J. Berzelius *.

Dalton's admirable researches have at last decided the dispute respecting the water of the atmosphere, which had lasted for nearly a whole century. The least absurd of the ideas advanced on the subject was, that the water became dissolved by the air, much in the manner as other solid bodies are dissolved by water, and that the aqueous meteors depended on alterations in the solvent capacity of the air, whereby the water is sometimes precipitated, producing clouds and rain, and at times dissolved, producing exhalations.

But Dalton has proved, that the water of the atmosphere is independent of the air; and that if the earth were deprived of the latter, it would nevertheless be surrounded by aqueous vapour, the extent of which would depend upon the degree of heat only; its increase in an increase of temperature being rather hindered than promoted by the air. The water contained in the air is in a gasiform state, mixed with the atmospheric air, just as in this the oxygen is mixed with the nitrogen, or as water is mixed with any other fluid. The quantity of water-gas in the air (as we have said) is in proportion to the temperature; and if the latter were immutable, the former would also continue the same; but perpetual changes of situations, circumstances, and temperature, produce continual alterations in this gas of the air, and from this alone are most of the aqueous meteors derived. Dalton, by a series of experiments, has calculated the quantity of water capable of maintaining a gasiform state, correspondent with every degree of the thermometer; and in a separate table determined these quantities according to the different columns of mercury they support. For instance, at — 15° † it is equivalent to a column of 0·064 inches, at —5° to 0·120, at 0° to 0·183, at + 15° to 0·422, at + 50° to 2·90, and at the boiling point to 25·0 inches, and this in vacuo as well as in the open air. But it seldom happens that the air is charged with water to the maximum of its temperature—a circumstance which renders the indications of the hygrometer highly useful and necessary. The hygrometer should express—To what column of mercury the water-gas of the air corresponds? and at the same time determine the absolute quantity of the gas; and, the temperature of the air being known—How much of this gas it can take up beyond what it already holds, and how soon the exhalation thereof can take place? Our usual hygrometers of hair, and of whalebone, are, in this respect, very imperfect: the results from them are not much to be relied on, having always a relation to the temperature in which the examination is made.

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* Translated from Berzelius's Philosophical Journal, 1808.

† What Thermometer does M. Berzelius use? Edit.

Dalton made use of a very plain instrument for his hygrometrical essays: he filled a long cylindrical glass vessel with cold well water, and when the dew appeared to coat the outside, he decanted the water, and wiped the glass well with linen, after which he returned the water, and this he repeated until the glass ceased to appear moist upon the introduction of the water; when he, by means of the thermometer, examined the temperature of the water so poured in: he then found the degree of heat at which the air might prove saturated (if I might so say) with the contained water-gas, and in consulting his tables he learned what column of mercury coincided therewith, he being already acquainted with the temperature of the air. This simple apparatus served all his purposes.

We nevertheless may easily see, that although this method is built upon a true principle, yet it will prove to be both inconvenient, tedious, and defective, as the precise temperature at which the glass should cease to appear moist cannot possibly be attained. Therefore, to obtain a greater certainty in the result, though scarcely with less trouble, I altered Dalton's plan in the following manner:

Let us suppose that the air which we are about to examine is at 20°, and that a glass of ordinary spring water, generally at 7° when recently taken from the spring, is bedewed in this air. The difference between the temperature of the air and the water is then equal to 13°. Should we take

12 glass vessels, in the first of which we mix 12 parts of this spring water with one part of water which has acquired the temperature of the air; in the second, 11 parts of the former with two of the latter, and so in a decreasing ratio throughout, we then obtain fluids which differ from each other by one degree of temperature. Of these we examine the first which does not bedew, and its temperature then precisely indicates the expansive capacity of the water-gas of the air. Should the air happen to be too dry, so as not to yield a deposition of water, then we might render it cooler by the addition of sal ammoniac which dissolves in it; and in winter we can increase the cold by snow, with salt or sal ammoniac. But even this method is slow and troublesome for hygrometrical investigations. I therefore determined to precipitate the water from the air by the thermometer itself, as follows:

The bulb, defended by a case of oiled silk, being immersed in the cold water, was taken up after having acquired the temperature of the water. It then became covered with vapour of water; I observed also the degree of the thermometer when the dew disappeared, and found the expansive capacity of the water-gas, according to Dalton's tables, tolerably near. But I also found that a very damp air at + 18° to + 20° produced a somewhat greater result than what it ought to be, because the thermometer, when much water is deposited on its bulb, proceeds little beyond the true point before all is evaporated. Besides, there is another circumstance which renders the use of the ordinary thermometer less certain, that, namely, a small portion of deposit is not accurately distinguished on the bulb. I therefore caused a thermometer bulb of steel to be made, greatly oblongated, its outside highly polished, and thereto attached a steel tube an inch long, wherein I closely cemented a thermometer tube, and made thereof an actual thermometer. This instrument completely answered my purpose. When the bulb with its oil case was immersed in the cooling mixture, and taken up as the mercury fell an inch, or every other inch according to circumstances, I at last arrived at a period when the bulb became covered with a quickly-passing coat of aqueous vapour, and the scale then expressed the degree of expansive capacity of the water-gas of the air with the greatest precision possible. This kind of hygrometer, besides the nicety of its results, also has the advantage that an experiment may be made without loss of time and trouble, and does not tire the observator like the former methods.

IX. *Materials for a History of the Prussiates.*

By M. Proust[*].

Part Second.

Some Precipitations by the simple Prussiate.

This prussiate yields, with metallic solutions, results different from those of the triple prussiate. Scheele had already remarked some of them, and the following came under my observation:

Silver. Triple prussiate: a white precipitate which soon became blue, on account of the white prussiate of iron which is mixed with that of silver.

Silver. Simple prussiate: a white curd which does not change.

Gold. Triple prussiate: nothing.

Gold. Simple: white precipitate, which becomes of a fine yellow.

If we heat the mixture, this precipitate, when heated, does not fulminate; it is a true prussiate of gold. When heated in a retort, it gives water, abundance of empyreumatic oil, carbonic acid gas, which burns with a blue flame, and a residue of gold mixed with charcoal powder. Upon looking over my notes I do not find ammonia mentioned, perhaps from neglect.

Molybdic Acid and *Oxide of Tungsten.*—The two prussiates yielded nothing in either of these cases.

Titanium. Triple prussiate: Prussian blue proceeding from the iron always retained by this oxide.

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[*] From *Annales de Chimie,* tome lx. p. 225.—For M. Proust's first paper, see the preceding volume.

Titanium. Simple ditto: yellow oxide of iron, such as the prussiate gives with the solutions of red oxide. I have never yet been able to obtain titanium exempt from iron.

Uranium. Triple prussiate: precipitate of a blood colour.

Uranium. Simple: whitish yellow.

Cobalt. Triple prussiate: precipitate of a grass green.

Cobalt. Simple: clear cinnamon colour.

Nickel. Triple prussiate: greenish white precipitate.

Nickel. Simple: yellowish white.

Manganese. Triple prussiate: precipitate of a pea-bloom colour.

Manganese. Simple: dirty yellow.

Copper. Triple prussiate: a fine crimson.

Copper. Simple: yellow.

White muriate of copper, or muriate the oxide of which is at the *minimum* dissolved in muriatic acid.—Triple prussiate: white precipitate, red inclined to crimson. We find that if this muriate was perfectly exempt from oxide at the *maximum*, the precipitate would be white. The solution of this muriate is like that of iron; it is difficult to keep it at the zero of hyper-oxidation, on account of the air.

The same muriate: simple prussiate: curdled precipitate, perfectly white. Some drops of potass take the prussic acid from it, and restore the yellow colour to it, which is the colour of the oxide of copper at the *minimum*.

Platina and the two prussiates: nothing.

Prussiate of Mercury.—This is obtained, as we already know, by treating the red oxide of mercury with Prussian blue. This salt crystallizes easily in tetrahedral prisms.

It is always opaque. It may contain potash, as we shall see presently, if there was any in the Prussian blue. It also contains oxide of iron; we may perceive this from the following experiment:

Heat some grains with muriatic acid in a small matrass, and white prussiate is precipitated.

In order to purge it from iron, we must boil its solution over red oxide several times: at every boiling it deposits oxide of iron; but this depuration is very tedious.

The prussiate of mercury changes its state on passing over the

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the red oxide, and seems to take a surcharge of it: for it does not any longer crystallize in prisms, but in small groups of very fine needle-like crystals. Their solutions also require more concentration: new solutions do not restore them to their first form.

This salt heated in a retort is very easily decomposed, and indeed totally, if we be not too hasty in heating it. It is sufficient to heat some grains of it in a tube three or four lines in diameter closed at one end. If, while it is heated, we present the open end to the flame, the prussic gas mixed with gaseous oxide takes fire. Its flame is red and blue, terminated by a yellowish aureola. One hundred grains of prismatic prussiate distilled yielded 72 grains of mercury, and on another occasion 72½.

The residue, being from eight to nine grains, was a mixture of charcoal and carbonate of potash. This is not surprising; the alkali cannot decompose the prussiate of mercury: it certainly belongs to the Prussian blue, which was that used in commerce.

The products from this distillation are ammonia and oil in abundance, besides a mixture of carbonic acid gas and carbonic acid.

There was apparently no prussiate with a base of oxide at the *minimum;* for the prussic acid, applied to mild mercury, and to the nitrate with a *minimum* base, eliminates a portion of mercury, and gives prussiate with a base of red oxide, the same as that obtained by treating this acid directly with red oxide.

The red oxide also decomposes the simple prussiate. Potass is also separated from it; and as it has no action upon the prussiate of mercury, the latter crystallizes in the mass. It also completely decomposes the triple prussiate, which requires long ebullitions: in this case the black oxide, the element of this salt, passes to the state of red oxide, and is deposited in ochre. A part of the mercury gives up to it the oxygen which it requires for this: hence it happens that we find it native with the ochre which is precipitated; but without the hyper-oxidation of the iron, which, as we know, diminishes the affinities of this metal, the oxide of mercury would not succeed perhaps in decomposing a combination so strong as that of the triple prussiate.

Diluted sulphuric acid has no action upon the prussiate of mercury even with heat, and not the slightest smell of prussic gas is perceived.

Potass saturates the sulphuric acid as the excipient of the prussiate, but precipitates nothing.

Concentrated sulphuric acid destroys the prussic acid, gives sulphureous acid, and thereby puts an end to every mean of comparison.

The nitric acid is not more successful, even after ebullition. We perceive very early a little nitrous gas, but it is certainly the black oxide containing the prismatic prussiate which occasions it: to conclude, the prussiate crystallizes in the mass of the acid. The alkalis saturate this last, and also precipitate nothing from it.

It does not elude the muriatic acid in the same way, however. There is a separation of prussic gas, a complete decomposition, and the prussiate is totally changed into corrosive sublimate. Alcohol also dissolves entirely the saline residue of this operation: finally, examined by the reagents, we find no longer any sublimate. Alcohol, as we know already, does not dissolve the prussiate of mercury.

Potash dissolves in abundance the prussiate of mercury by means of heat. This salt crystallizes in it upon cooling. Alcohol separates it from it, and we recover it entirely.

The muriate of tin at the *minimum*, and hydro-sulphuretted water instantly decompose this prussiate, and the prussic acid becomes free.

We have seen that the muriatic acid acted efficaciously upon this prussiate. From this it should seem that the sal ammoniac which presents to the prussic acid a principle capable of uniting with it, should be able to exchange the other with the mercury: this does not happen, however. If we heat a solution of mercurial prussiate, and of muriate of ammonia, there is nothing new. Alcohol separates them entirely. Potash and limewater precipitate nothing from them; not an atom of corrosive sublimate; and the green sulphate, which could not fail to form prussiate of iron with that of ammonia, were the latter present, does not undergo the least change.

Prussic Gas.—Twenty drachms of triple prussiate heated in a retort with a sufficient quantity of weak sulphuric acid, charged four ounces of alcohol with about twenty-four grains. I kept the alcohol in a bell glass over the bath of mercury: the gas is dissolved rapidly, but it would have taken much more. The water of the intermediate receiver was also surcharged with it: the smell was pungent and suffocating, and its taste very strong of almond kernel. This water did not disturb barytes. The gas always tends to separate from it, and continually elevates the stopper: if we plunge a small matrass of it into hot water, it is rapidly separated, and burns at the opening of it: if we bring the flame of a candle to it, we perceive smoke; doubtless because a part of the carbon escapes, as in the combustion of the volatile oils.

The prussic acid dissolved in water and well corked is decomposed by itself. It is coloured yellow in four or five months. It loses its smell gradually, becomes turbid, and deposits a sediment of a coffee colour, which, after having been heated, presents all the characters of carbon.

It gives by distillation a little water, with prussic and ammoniacal acid. The carbon is azotized; and it has resumed one of the principles which the acid abandons by its destruction; for I have heated it with carbonate of potash, and it gave me a lixivium proper for making Prussian blue.

But while the carbon is separated by retaining azote, the greatest part of this last, added to the hydrogen, is constituted in ammonia: we also find it in the yellow liquor, with the rest of the acid which has escaped its destruction.

The prussic gas, dissolved in water, does not disturb the solution of green sulphate: but when it has passed through the changes we are about to mention, it disturbs it and gives a blue, because the ammonia of new formation concurs to it.

Lastly, this liquor when distilled gives prussiate of ammonia, and there is no longer any thing in it but atoms of a charry matter which are deposited. It would have been important

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important to have ascertained if the carbonic acid existed there with ammonia, but I neglected it at the time. I shall, however, return to that subject.

Prussic alcohol is preserved extremely well: we might even conclude from this, with some foundation, that if alcohol is more proper than water for its solution and preservation, the prussic gas, considered besides in its qualities of being aromatic and inflammable, is perhaps more visibly allied to oily combustible products, and of a complex nature, than to saline substances.

It results from these facts, in the first place, that there is only one prussiate of mercury, being that of which the base is at the *maximum*. Secondly, that all this exaltation of affinities which the prussic acid borrows from the black oxide, when it is requisite to use potash, or the red oxide of iron, and upon which Berthollet has insisted with so much justice, ceases to be necessary to it, if it is in contact with oxides of gold, silver, copper, cobalt, nickel, uranium, mercury, &c. We see, in fact, that with regard to the latter, this acid, the affinities of which are so indolent, and so little deserve the title of affinities, has however no occasion for black oxide, in order to furnish with mercury a saline combination, very soluble, very crystallizable, endowed, in a word, with all the characters which distinguish the most perfect compounds. Add to these mysterious circumstances, its preference to mercury over all the alkalis, and its not yielding its oxide either to the nitric acid or to the sulphuric acid, which their power raises so much above it; and lastly, its only yielding to the muriatic acid, which we know to be in so many respects inferior to the sulphuric and nitric acids.

Lixivium of Animal Charcoal.—Equal parts of charcoal of blood, and of carbonate of potash, made red-hot in a covered crucible, have always furnished me with the richest lixivium.

Thinking that the carbonic acid might be an obstacle to the saturation of the potash, I added lime to the mixture, but the lixivium was not improved by it.

I kept red-hot for half an hour, a mixture of 144 grains of charcoal, and the same quantity of carbonate. The lixivium being finished, the charcoal extracted was only 104 grains: 40 grains were destroyed.

These 104 grains were again treated with 144 of carbonate: they were reduced to 62; loss 42.

The lixivium of these two experiments was saturated with the solution of the sulphate of iron used in commerce: the blue of the first, after the colour was struck, was double in volume to that of the second.

In order to ascertain the influence of temperature, I tried three mixtures of equal quantities. The first was kept red-hot for half an hour, the second one hour, and the third an hour and a quarter. The first lixivium gave very little blue; the two last gave a great deal, and much about the same quantity. These results prove either that the simple prussiate, being that which predominates in the lixiviums, is preserved in the midst of the carbonaceous alkaline mass, or that it is reproduced in proportion as it is destroyed.

The charcoal of blood, pulverized, liquefies in the air: when washed, it gives sea salt, and carbonate of soda holding a little prussic acid in solution.

The charcoal of blood, when treated in this way a second time, still yields blue, but in small quantity; a third, less sensibly; a fourth, not at all. This charcoal, when made red-hot, is incinerated with much facility without exhaling the ammoniacal smell. It appears, that in proportion as it loses the azote, it becomes more combustible, and resembles more closely vegetable charcoals: the nitric acid, however, does not inflame it. The azote being susceptible of forming concrete combinations capable of resisting a high temperature, what would be the influence of animal charcoal in the formation of steel?

Equal parts of charcoal of blood, washed, and of potash rendered caustic by lime, yielded, by the distillation of the simple prussiate of ammonia, plenty of gas, which had the same smell with the prussic, and which burns red.

Equal parts of this same charcoal, and of oxide of manganese, yielded carbonate and prussiate of ammonia.

The desire of forming ammonia to some profit, led me to the following experiment: I distilled the following mixture: charcoal of blood, six drachms; argil and sea salt, each two drachms; but the produce in sal ammoniac was much below my expectations.

All the vegetable charcoals azotized are proper for making Prussian blue. Thus those of gluten, of chick peas, indigo, and of pit-coal, have yielded dyeing lixiviums, sometimes mixed with hydro-sulphuret: those of sugar-cane and of milk do not give any indication of blue.

Charcoal of chesnuts and of brush-wood, which are preferred at the founderies to any other kind, because they have the advantage of being extinguished the moment the bellows cease, do not owe this to azote, for their lixiviums do not contain any thing prussic in them.

Cream of tartar made red-hot gives a lixivium, which does not give the least blue: nor did two parts of cream of tartar and one of sal ammoniac; but one part of sal ammoniac with four of cream of tartar yield a lixivium which contains simple prussiate. It gives blue with the green sulphate of commerce. Cream of tartar and nitre of soda, nothing.

This result proves clearly that it is by the azote alone that animal are preferable to vegetable charcoals. It also results from this, that if we can at any time discover some azotized combination, more capable of supporting a strong heat than the ammoniacal salts, we might succeed in forming the prussic acid in a manner perhaps less laborious than by the animal charcoals.

Examination of the Lixiviums.—By distillation they give continually prussic acid and ammonia: we have seen the origin of this a little higher.

They contain carbonate of potash in a great quantity.
Simple prussiate of potash.
Triple ditto.
Sulphate of potash.
Phosphate of lime.
Sulphur.

They deposit phosphate of lime in proportion as we evaporate them; I know not how it is formed.

If

If we saturate a portion of lixivium with sulphate of iron, and examine the blue liquor formed by it, we discover phosphate of iron. This phosphate induced Westrumb to think that the prussic acid was phosphoric.

Alcohol applied to concentrated lixiviums takes simple prussiate from them; but it appears difficult to purify them by this means. The triple prussiate remains in the lixivium with the carbonate.

Of these two prussiates, one only can give Prussian blue with solutions of red oxide; namely, the triple prussiate, because it is provided with black oxide. The other cannot, because it has not the black oxide: but it does so, and becomes triple prussiate, as soon as we mix the lixiviums with the sulphate of iron of commerce: and consequently, if we use sulphate completely red, we shall have infinitely less Prussian blue, because, the black oxide being wanting, it could not become triple prussiate and give blue with this sulphate. Two experiments will render this apparent.

I divided a lixivium into two equal parts; one part was precipitated with red sulphate, and the other with the green sulphate of commerce. The excess of oxides being separated, the blue of the second was in proportion to that of the first as four to one.

The first lixivium, when filtered, exhaled a strong smell of almonds. I saturated it with potash, in order to fix once more the free prussic acid: when tried afterwards with red sulphate, it did not give one atom of blue; but with the green sulphate it gave abundance. We may therefore conclude that, without the aid of black oxide, a carbonaceous lixivium would not give with solution of red oxide the whole of the blue which it might. From this would arise the loss that might be suffered of all the simple prussiate contained in a lixivium if we only used a sulphate the oxide of which was completely red; and from this proceeded the mistake into which I fell when I advised it. I did not reflect that, if the green sulphate has the inconvenience of giving pale prussiate, the oxygen of the air soon remedies this defect; but it has the essential advantage of furnishing to the simple prussiate the portion of black oxide which it requires in order to convert it into a triple salt, and may afterwards furnish blue with the red solutions. In this way practice had preceded theory in accomplishing an object; but practice also becomes in its turn a rational formula, from the moment that it is confirmed by theory. Two other experiments strengthen this demonstration.

The lixiviums are generally precipitated with a solution of four parts of alum, and one of the sulphate of commerce.

I divided one of these solutions into two parts: the one was hyper-oxidated by the oxy-muriatic acid, and the other not. I afterwards saturated them with carbonaceous lixivium. The common solution furnished blue in abundance, but the hyper-oxidated gave a very pale precipitate, which was only a little blue mixed with a great deal of alumine. This experiment does not differ essentially from the preceding. It has only the advantage of showing that the alum is but a passive ingredient in the formation of Prussian blue.

It is not the same therefore with the lixiviums of the manufacturer, as it is with an alkali passed over Prussian blue: the latter will always give blue in abundance, because it comes out of the operation tripled, but the lixiviums do not. They could not give it but in proportion to the triple prussiate which they contain: it is in order to increase it, or to raise their simple prussiate to the same degree, that it is indispensable to use, if not a sulphate rigorously green, at least one which contains a certain quantity of green; and this is precisely the usual quality of that of commerce, however old it may be.

These details also explain to us, that if the lixiviums contain a portion only of tripled prussiate, it is because charcoal of blood has not iron enough to furnish for raising all the simple prussiate formed during calcination to the triple degree, or rather because a part of the latter again becomes simple prussiate by the loss of its oxide, as we have seen happen to it when heated alone. Of these two opinions, however, I adhere to the last, because I have remarked, that the charcoal which served the lixiviums gives ashes which always contain much iron: thus, in the calcination of the alkaline carbonaceous mixtures, we cannot presume with

reason,

reason, that it is iron which is wanting in the prussiate: and even if we reflect on it, it is astonishing to see that the triple prussiate, which certainly exists in the lixiviums, could defend its oxide against the effects of the charcoal, which tends to reduce it continually. In short, all this part of the subject is very obscure. We do not know the period at which the prussic acid is formed, if it be destroyed in order to be reproduced, nor finally the degree of heat to be applied to the boilers, in order to obtain the greatest possible quantity of the one or the other of the prussiates which it is the object of the manufacturer to obtain.

The existence of the triple prussiate is clearly demonstrated in the lixiviums, by the following experiment:

Saturate a lixivium with dilute sulphuric acid: carbonic acid is first set free, afterwards comes prussic acid from the free prussiate: but it must be afterwards heated: we then obtain the triple prussiate, and the white prussiate of iron is set free. Besides this, the old concentrated lixiviums deposit octahedral crystals of triple prussiate.

The prussic lixivium has two distinct tastes; the one of potash, and the other of kernels: and from this latter taste we judge of its quality. If it perfumes the mouth but feebly it is not good; and either the mixture has not been sufficiently heated, or the charcoal has been used too sparingly. I think also, that the calcination of the mixtures in the open air ought not to contribute to the augmentation of the prussiates, and that it would perhaps be more advantageous, and less troublesome, to heat them in close crucibles placed in a reverberating furnace, since it is in other respects certain that agitation is by no means necessary to the success of this operation.

When we have occasion to concentrate the lixiviums, in order to diminish their volume, or in order to preserve them, we should begin before every thing, as observed by Curadeau, in placing the simple prussiate beyond the reach of destruction: this may be at once effected by pouring green sulphate into it by small portions at a time. The green sulphate is completely dissolved: the lixivium at first becomes red, and then yellow: an excess of sulphate does not alter it at all, because the potash which predominates reduces it to oxide. The latter is then deposited without passing to the state of prussiate. To attain this, it must be accompanied by an acid, for the oxide in question (being only at the minimum) has no effect upon the triple prussiate. The following experiment clearly demonstrates the advantages of this method:

I divided a lixivium into two equal parts: the one was prepared or tripled by green sulphate, the other not. I afterwards distilled them: the former gave no suspicion of the presence of ammonia, and the latter furnished it as usual. It is indispensable, therefore, to prepare lixiviums before concentrating them. Lastly, neither the red oxide, nor its sulphate, as Scheele experienced, can be dissolved in the simple prussiate, and give it the quality of triple prussiate: this oxide, although fit to become the base of Prussian blue, cannot decompose the triple prussiate: it must necessarily be used when dissolved in an acid.

Recapitulation.

The prussic acid is composed of carbon, azote, and hydrogen, in proportions with which we are not yet acquainted. Considering the great quantity of charcoal, however, which it leaves after its destruction in several cases, we may conjecture that it contains carbon in a greater proportion than the two other substances. No person has supposed that oxygen entered into its composition; and in truth the well-known affinities of its three elements, added to the circumstances attending its formation, have not as yet permitted us to hazard the opinion.

The prussic acid, when by itself, has very few of the general qualities of the acids. It has not a sour taste: it does not redden turnsole: it is not so easily dissolved in water (the true solvent of the acids) as it is in alcohol: in this last solvent it is decomposed even spontaneously, and without the assistance of the external air. It forms with the alkalis combinations so imperfect, that we find in them, almost in a state of perfect separation, the specific properties of the component parts, and the carbonic acid, the weakest of all, is sufficient to decompose them. In a word, its combustibility, taste, aromatic smell, its being generated in the midst of volatile oils, and its preservation in alcohol, exhibit qualities which much more strongly resemble oily and inflammable productions than saline substances.

The prussic acid, notwithsthanding its trifling saline energy, has a powerful action on the major oxide of mercury: it furnishes with this oxide a saline combination, so well characterized in its attributes, that we are compelled to acknowledge that it acts in certain circumstances like the most powerful acid. Nothing in fact is wanting to the prussiate of mercury, to entitle it to be ranked among the most perfect of metallic salts: it will perhaps astonish some chemists, to see that it refuses to be united to the minor oxide; but by a concurrence of affinities, of which we have other examples, it raises it to the state of major oxide, by eliminating a part of the metal, in order to form, with the other, prussiate of mercury.

The prussic acid has no action upon the red oxide of iron; but it attacks the black oxide, and produces white prussiate with it. This prussiate, it is true, is not absolutely white, the difficulty of preparing, with green sulphate, a precipitate at the zero of hyper-oxidation, not permitting it: thus, it is always greenish; but as, upon drying, it becomes perfect Prussian blue, we cannot doubt that the prussic acid, plus the base of the green sulphate, will give, all perturbation being out of the question, a prussiate equally white with that which we obtain by more easy means.

Prussian blue is not a simple combination, as has been thought. The following observation will sufficiently prove this assertion: We know, for instance, that the basis of this blue is red oxide: but if this oxide be sufficient of itself for making Prussian blue, why should not the prussic acid and the red oxide furnish it? Why should not the solutions of this oxide, and the simple alkaline prussiates, give it also? There must necessarily be another element in Prussian blue: the following facts clearly demonstrate this: When we apply potash to Prussian blue we obtain a yellow crystallizable salt, which has always a constant proportion of black oxide.

If we employ the yellow prussiate in reproducing Prussian blue, this oxide re-enters with the prussic acid into the new combination. The black oxide is therefore an element necessary to the formation both of the crystallizable prussiate and of the Prussian blue, and likewise of all the metallic prussiates prepared with the triple prussiate of potash.

There are metals which are susceptible of forming simple and triple prussiates, such as copper, silver, manganese, cobalt, nickel, uranium, &c. There are some which give simple prussiate, such as gold, mercury, &c. There are some also which give triple prussiate only, such as iron, &c. Lastly, some of them do not appear susceptible either of the one or the other. But, with the exception of Prussian blue and prussiate of mercury, all the rest are little known, and merit examination. The black oxide united to the prussic acid may pass from the one combination to the other without changing its state: the base of this combination may even be raised from the *minimum* to the *maximum*, without the black oxide on that account taking any part in this change. The combination of the acid with this oxide is maintained by so powerful an affinity that the alkaline hydro-sulphurets cannot separate them, or rather they cannot touch the oxide in the triple prussiate of potash, or in Prussian blue.

The prussic acid united to this portion of black oxide, which enables it to form triple, alkaline, or metallic prussiates, is a peculiar combination, the existence of which is not doubtful; but with which we are not as yet acquainted, except in these prussiates alone.

The triple prussiate of potash cannot undergo a red heat without losing the black oxide, and consequently without being reduced to the state of simple prussiate.

The simple prussiate is also decomposed, but by a far lower temperature: its acid is destroyed, and reduced to ammonia and carbonic acid: it is the destruction of this salt by the heat of ebullition, which degrades the lixiviums for preparing Prussian blue.

The simple prussiate assumes the character of triple prussiate,

siate, as soon as we present to it either the black oxide, or a salt with a base of black oxide, and acquires, besides the advantage of crystallizing, that of being no longer decomposable by the heat of ebullition.

This prussiate, which was the test liquor so long wanted by chemists, does not give Prussian blue with solutions of red oxide; but it gives them if they contain black oxide, because its acid is attached immediately to that portion of the same oxide, which ought to serve as an intermedium between it and the red oxide.

The triple prussiate of iron, or Prussian blue, strongly heated, is reduced to ammonia, to the two gases of carbonic acid and gaseous oxide, to iron (*fer aceré*) and to charcoal.

The prussiate of mercury gives the same products by its decomposition, besides a certain portion of oil.

The charry lixiviums contain but little triple prussiate, and a great deal of simple prussiate. They must not be concentrated without having in the first place strengthened the constitution of the simple prussiate by an addition of black oxide, or of green sulphate.

In order to obtain from these lixiviums the whole of the Prussian blue which they are susceptible of giving, it is indispensably requisite to use a sulphate, a portion of which at least is green: without this precaution, the simple prussiate contained in them could not furnish blue with a sulphate, the base of which was completely red.

To conclude, if this memoir be compared with that of Scheele, it will be found that all the facts above stated were perfectly well known to him: but they appeared to me to require some further elucidation; and with this view I lay my present memoir before the public.

X. *Observations of a Comet, made with a View to investigate its Magnitude, and the Nature of its Illumination.*

By William Herschel, *LL.D. F.R.S.*[*]

The comet, which we have lately observed, was pointed out to me by Mr. Piggot, who discovered it at Bath the 28

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[*] From Philosophical Transactions for 1808, Part II.

28th of September; and the first time I had an opportunity of examining it was the 4th of October, when its brightness to the naked eye gave me great hopes to find it of a different construction from many I have seen before, in which no solid body could be discovered with any of my telescopes.

In the following observations, my attention has been directed to such phænomena only, as were likely to give us some information relating to the physical condition of the comet: it will therefore not be expected that I should give an account of its motion, which I was well assured would be most accurately ascertained at the Royal Observatory at Greenwich.

The different parts of a comet have been generally expressed by terms that may be liable to misapprehension, such as the head, the tail, the coma, and the nucleus; for in reading what some authors say of the head, when they speak of the size of the comet, it is evident that they take it for what is often called the nucleus. The truth is, that inferior telescopes, which cannot show the real nucleus, will give a certain magnitude of the comet, which may be called its head; it includes all the very bright surrounding light; nor is the name of the head badly applied, if we keep it to this meaning; and since, with proper restriction, the terms which have been used may be retained, I shall give a short account of my observations of the comet, as they relate to the above-mentioned particulars, namely, the nucleus, the head, the coma, and the tail, without regarding the order of the time when they were made. The date of each observation, however, will be added, that any person who may hereafter be in possession of more accurate elements of the comet's orbit, than those which I have at present, may repeat the calculations in order to obtain a more accurate result.

Of the Nucleus.

From what has already been said, it will easily be understood, that, by the nucleus of the comet, I mean that part of the head which appears to be a condensed or solid body, and in which none of the very bright coma is included. It should be remarked, that from this definition it follows, that when the nucleus is very small, no telescope, but what has light and power in an eminent degree, will show it distinctly.

Observations.

Oct. 4, 1807. 10-feet reflector. The comet has a nucleus, the disk of which is plainly to be seen.

Oct. 6. I examined the disk of the comet with a proper set of diaphragms, such as described in a former paper*, in order to see whether any part of it were spurious; but when the exterior light was excluded, so far from appearing larger, as would have been the case with a spurious disk, it appeared rather diminished for want of light; nor was its diameter lessened when I used only the outside rays of the mirror. The visible disk of the comet therefore is a real one.

Oct. 4. I viewed the comet with different magnifying powers, but found that its light was not sufficiently intense to bear very high ones. As far as 200 and 300, my 10-feet reflector acted very well, but with 400 and 500 there was nothing gained, because the exertion of a power depending on the quantity of light was obstructed†, which I found was here of greater consequence than the increase of magnitude.

Illumination of the Nucleus.

Oct. 4, 6h. 15′. The nucleus is apparently round, and equally bright all over its disk. I attended particularly to its roundness.

Oct. 18. The nucleus is not only round, but also every where of equal brightness.

Oct. 19. I see the nucleus again, perfectly round, well defined, and equally luminous. Its brilliant colour in my ten-feet telescope is a little tinged with red; but less so than that of Arcturus to the naked eye.

Magnitude of the Nucleus.

Oct. 26. In order to see the nucleus as small as it really is, we should look at it a long while, that the eye may gra-

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* See Phil. Trans. for 1805, p. 53. Use of the Criterion.

† See Phil. Trans. for 1800, p. 78.

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dually lose the impression of the bright coma which surrounds it. This impression will diminish gradually; and when the eye has got the better of it, the nucleus will then be seen most distinctly, and of a determined magnitude.

Oct. 4. With a seven-feet reflector I estimated the diameter of the nucleus of the comet at first to be about five seconds; but soon after I called it four, and by looking at it longer, I supposed it could not exceed three seconds.

Oct. 6. 10-feet reflector, power 221. The apparent disk of the comet is much less than that of the Georgian planet, which being an object I have seen so often with the same instrument, and magnifying power, this estimation from memory cannot be very erroneous.

Oct. 5. Micrometers for measuring very small diameters, when high magnifying powers cannot be used, being very little to be depended upon, I erected a set of sealing-wax globules upon a post at 2422 inches from the object mirror of my ten-feet reflector, and viewed them with an eye glass, which gives the instrument a power of 221, this being the same which I had found last night to show the nucleus of the comet well. I kept them in their place all the day, and reviewed them from time to time, that their magnitudes might be more precisely remembered in the evening, when I intended to compare the appearance of the nucleus with them.

On examining the comet, I found the diameter of its nucleus to be certainly less than the largest of my globules, which, being ·0466 of an inch, subtended an angle of 3″·97 at the distance of the telescope in the day time.

Comparing the nucleus also with the impressions which the view of the second and third had left in my memory, and of which the real diameters were ·0325 and ·0290 of an inch, and magnitudes at the station of the mirror 2″·77 and 2″·47, I found, that the comet was almost as large as the second, and a little larger than the third.

Oct. 18. The nucleus is less than the globule which subtends 2″·77.

Oct. 19. The air being uncommonly clear, I saw the comet 40 minutes after five; and being now at a considerable altitude, I examined it with 289, and having but very lately reviewed my globules, I judged its diameter to be not only less than my second globule, but also less than the third: that is, less than 2″·47.

Oct. 6. The 20-feet reflector, notwithstanding its great light, does not show the nucleus of the comet larger than the ten-feet, with an equal magnifier, makes it.

Oct. 28. My large ten-feet telescope, with the mirror of 24 inches in diameter, does not increase the size of the nucleus.

Oct. 6. Being fully aware of the objections that may be made against the method of comparing the magnitude of the nucleus of the comet with objects that cannot be seen together, I had recourse to the satellites of Jupiter for a more decisive result, and with my seven-feet telescope, power 202, I viewed the disk of the third satellite and of the nucleus of the comet alternately. They were both already too low to be seen very distinctly; the diameter of the nucleus however appeared to be less than twice that of the satellite.

Oct. 18. With the ten-feet reflector, and the power 221, a similar estimation was made; but the light of the moon would not permit a fair comparison.

Oct. 19. I had prepared a new ten-feet mirror, the delicate polish of my former one having suffered a little from being exposed to damp air in nocturnal observations. This new one being uncommonly distinct, and the air also remarkably clear, I turned the telescope from the comet to Jupiter's third satellite, and saw its diameter very distinctly larger than the nucleus of the comet. I turned the telescope again to the comet, and as soon as I saw it distinctly round and well defined, I was assured that its diameter was less than that of the satellite.

6h. 20′. I repeated these alternate observations, and always found the same result. The night is beautifully clear, and the moon has not yet risen to interfere with the light of the comet.

Nov. 20. With a seven-feet reflector, and power only 75, I can also see the nucleus; it is extremely small, being little more than a mere point.

Of the Head of the Comet.

When the comet is viewed with an inferior telescope, or if the magnifying power, with a pretty good one, is either much too low, or much too high, the very bright rays immediately contiguous to the nucleus will seem to belong to it, and form what may be called the head.

Oct. 19. I examined the head of the comet with an indifferent telescope, in the manner I have described, and found it apparently of the size of the planet Jupiter, when it is viewed with the same telescope and magnifying power.

With a good telescope, I saw in the centre of the head a very small well-defined round point.

Nov. 20. The head of the comet is now less brilliant than it has been.

Of the Coma of the Comet.

The coma is the nebulous appearance surrounding the head.

Oct. 19. By the field of view of my reflector, I estimate the coma of the comet to be about six minutes in diameter.

Dec. 6. The extent of the coma, with a mirror of 24 inches diameter, is now about 4′ 45″.

Of the Tail of the Comet.

Oct. 18. 7h. With a night glass, which has a field of view of nearly 5°, I estimated the length of the tail to be 3°¾; but twilight is still very strong, which may prevent my seeing the whole of it.

Nov. 20. The tail of the comet is still of a considerable length, certainly not less than 2½ degrees.

Oct. 26. The tail of the comet is considerably longer on the south-preceding, than on the north-following side.

It is not bifid, as I have seen the comet of 1769 delineated by a gentleman who had carefully observed it*.

Oct. 28. 7-feet reflector. The south-preceding side of the tail in all its length, except towards the end, is very well defined; but the north-following side is every where hazy

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* Dr. Lind of Windsor.

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and irregular, especially towards the end; it is also shorter than the south-preceding one.

The shape of the unequal length of the sides of the tail, when attentively viewed, is visible in a night glass, and even to the naked eye.

Oct. 31. 10-feet reflector. The tail continues to be better defined on the south-preceding than on the north-following side.

Dec. 6. The length of the tail is now reduced to about 23′ of a degree.

Of the Density of the Coma and Tail of the Comet.

Many authors have said, that the tails of comets are of so rare a texture, as not to affect the light of the smallest stars that are seen through them. Unwilling to take any thing upon trust, that may be brought to the test of observation, I took notice of many small stars, that were occasionally covered by the coma and the tail, and the result is as follows.

Oct. 26. 6h. 15′. Large 10-feet reflector, 24 inches aperture. A small star within the coma is equally faint with two other stars that are on the north-following side of the comet, but without the coma.

7h. 30′. The coma being partly removed from the star, it is now brighter than it was before.

Oct. 31. 6h. 5′. 10-feet reflector. A star in the tail of the comet, which we will call *a*, is much less bright than two others, *b* and *c*, without the tail.

Two other stars, *d* and *e*, towards the south of *b* and *c*, are in the following skirts of the tail, and are extremely faint.

7h. 20′. The star *e* is now considerably bright, the tail having left it, while *d*, which is rather more involved than it was before, is hardly to be seen.

7h. 50′. The star *a*, toward which the comet moves, is involved in denser nebulosity than before, and is grown fainter.

d is involved in brighter nebulosity than before, but being near the margin, it will soon emerge.

8h. 35′. Being still more involved, the star *a* is now hardly visible.

e is quite clear of the tail, and is a considerable star; *d* remains involved.

9h. 10′. The star *d* is also emerged, but the comet is now too low to estimate the brightness of stars properly.

Nov. 25. 7h. 35′. There is a star *a* within the light of the tail, near the head of the comet, equal to a star *b* situate without the tail, but near enough to be seen in the field of view with *a*. The path of the head of the comet leads towards *a*, and a more intense brightness will come upon it.

8h. 46′. The star *a* is now involved in the brightness near the head of the comet, and is no longer visible, except now and then very faintly, by occasional imperfect glimpses; but the star *b* retains its former light.

Nebulous Appearance of the Comet.

Dec. 6. The head of the comet, viewed with a mirror of 24 inches diameter, resembles now one of those nebulæ, which in my catalogues would have been described, "a very large, brilliant, round nebula, suddenly much brighter in the middle."

Dec. 16. 7-feet reflector. The night being fine, and the moon not risen, the comet resembles "a very bright, large, irregular, round nebula, very gradually much brighter in the middle, with a faint nebulosity on the south-preceding side."

Jan. 1, 1808. 7-feet. "Very bright, very large, very gradually much brighter in the middle."

If I had not known this to be a comet, I should have added to my description of it as a nebula, that the centre of it might consist of very small stars; but this being impossible, I directed my ten-feet telescope with a high power to the comet, in order to ascertain the cause of this appearance; in consequence of which I perceived several small stars shining through the nebulosity of the coma.

Jan. 14. 7-feet. "Bright, pretty large, irregular round, brighter in the middle."

Feb. 2. 10-feet, 24-inch aperture. "Very bright, large, irregular round, very gradually much brighter in the middle." There is a very faint diffused nebulosity on the north-preceding side; I take it to be the vanishing remains of the comet's tail.

Feb. 19. Considerably bright; about $\frac{1}{7}$th of the field = 3′ 26″ "in diameter, gradually brighter in the middle." The faint nebulosity, in the place where the tail used to be, still projects a little further from the centre than in other directions.

Feb. 21. Less bright than on the 19th; nearly of the same size: gradually brighter in the middle. The nebulosity still a little projecting on the side where the tail used to be.

Result of the foregoing Observations.

From the observations which are now before us, we may draw some inferences, which will be of considerable importance with regard to the information they give us, not only of the size of the comet, but also of the nature of its illumination.

A visible, round, and well defined disk, shining in every part of it with equal brightness, elucidates two material circumstances; for since the nucleus of this comet, like the body of a planet, appeared in the shape of a disk, which was experimentally found to be a real one, we have good reason to believe that it consists of some condensed or solid body, the magnitude of which may be ascertained by calculation. For instance, we have seen, that its apparent diameter, the 19th of October, 6h. 20′, was not quite so large as that of the third satellite of Jupiter. In order therefore to have some idea of the real magnitude of our comet, we may admit that its diameter at the time of observation was about 1″, which certainly cannot be far from truth. The diameter of the third satellite of Jupiter, however, is known to have a permanent disk, such as may at any convenient time be measured with all the accuracy that can be used; and when the result of such a measure has given us the diameter of this satellite, it may by calculation be brought to the distance from the Earth at which, in my observation, it was compared with the diameter of the comet, and thus more accuracy, if it should be required, may be obtained. The following result of my calculation, however, appears to me quite sufficient for the purpose of general information. From the perihelion distance 0·647491, and the rest of the given elements of the comet, we find, that its distance from the ascending node on its orbit at the time of observation was 73° 45′ 44″; and having also the Earth's distance from the same node, and the inclination of the comet's orbit, we compute by these data the angle at the sun. Then by calculating in the next place the radius vector of the comet, and having likewise the distance of the Earth from the sun, we find by computation, that the distance of the comet from the Earth at the time of observation was 1·169192, the mean distance of the Earth being 1. Now since the disk of the comet was observed to subtend an angle of 1″, which brought to the mean distance of the Earth gives 1′·169, and since we also know that, the Earth's diameter, which, according to Mr. Dalby, is 7913·2 miles*, subtends at the same distance an angle of 17″·2, we deduce from these principles the real diameter of the comet, which is 538 miles.

Having thus investigated the magnitude of our comet, we may in the next place also apply calculation to its illumination. The observations relating to the light of the comet were made from the 4th of October to the 19th. In all which time the comet uniformly preserved the appearance of a planetary disk fully enlightened by the sun: it was every where equally bright, round, and well defined on its borders. Now as that part of the disk which was then visible to us could not possibly have a full illumination from the sun, I have calculated the phases of the comet for the 4th and for the 19th; the result of which is, that on the 4th the illumination was 119° 45′ 9″, as represented in Plate II. fig. 4, and that on the 19th it had gradually increased to 124° 22′ 40″, of which a representation is given in fig. 5. Both phases appear to me sufficiently defalcated, to prove that the comet did not shine by light reflected from the sun only; for, had this been the case, the deficiency, I think, would have been perceived, notwithstanding the smallness of the object. Those who are acquainted with my experiments on small silver globules* will easily admit, that the same telescope which could show the spherical form of balls, that subtended only a few tenths of a second in diameter, would surely not have represented a cometary disk as circular, if it had been as deficient as are the figures which give the calculated appearances.

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* See Philosophical Transactions for 1791, p. 239. Mr. Dalby gives the two semiaxes of the Earth, from a mean of which the above diameter 7913·1682 is obtained.

If these remarks are well founded, we are authorised to conclude, that the body of the comet on its surface is self-luminous, from whatever cause this quality may be derived. The vivacity of the light of the comet also had a much greater resemblance to the radiance of the stars, than to the mild reflection of the sun's beams from the moon, which is an additional support of our former inference.

The changes in the brightness of the small stars, when they are successively immerged in the tail or coma of the comet, or cleared from them, prove evidently, that they are sufficiently dense to obstruct the free passage of star-light. Indeed if the tail or coma were composed of particles that reflect the light of the sun, to make them visible we ought rather to expect that the number of solid reflecting particles, required for this purpose, would entirely prevent our seeing any stars through them. But the brightness of the head, coma, and tail alone, will sufficiently account for the observed changes, if we admit that they shine not by reflection, but by their own radiance; for a faint object projected on a bright ground, or seen through it, will certainly appear somewhat fainter, although its rays should meet with no obstruction in coming to the eye. Now, as in this case we are sure of the bright interposition of the parts of the comet, but have no knowledge of floating particles, we ought certainly not to ascribe an effect to a hypothetical cause, when the existence of one, quite sufficient to explain the phænomena, is evident.

If we admit that the observed full illumination of the disk of the comet cannot be accounted for from reflection, we may draw the same conclusion, with respect to the brightness of the head, coma, and tail, from the following consideration. The observation of the 2d of February mentions, that not only the head and coma were still very bright, but that also the faint remains of the tail were still visible; but the distance of the comet from the Earth, at the time of observation, was nearly 240 millions of miles*, which proves, I think, that no light reflected from floating particles could possibly have reached the eye, without supposing the number, extent, and density of these particles far greater than what can be admitted.

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* Philosophical Transactions for 1805, p. 38, the 5th experiment.

My last observation of the comet, on the 21st of February, gives additional support to what has been said; for at the time of this observation the comet was almost 2·9 times the mean distance of the sun from the Earth †. It was also nearly 2·7 from the sun ‡. What chance then could rays going to the comet from the sun, at such a distance, have to be seen after reflection, by an eye placed at more than 275 millions of miles § from the comet? And yet the instant the comet made its appearance in the telescope, it struck the eye as a very conspicuous object.

The immense tails also of some comets that have been observed, and even that of the present one, the tail of which, on the 18th of October, was expanded over a space of more than nine millions of miles ||, may be accounted for more satisfactorily, by admitting them to consist of radiant matter, such as, for instance, the aurora borealis, than when we unnecessarily ascribe their light to a reflection of the sun's illumination thrown upon vapours supposed to arise from the body of the comet.

By the gradual increase of the distance of our comet, we have seen, that it assumed the resemblance of a nebula; and it is certain, that had I met with it in one of my sweeps of the zones of the heavens, as it appeared on either of the days between the 6th of December and the 21st of February, it would have been put down in the list I have given of nebulæ. This remark cannot but raise a suspicion, that some comets may have actually been seen under a nebulous form, and as such have been recorded in my catalogues; and were it not a task of many years' labour, I should undertake a review of all my nebulæ, in order to see whether any of them were wanting, or had changed their place; which certainly would be an investigation, that might lead to very interesting conclusions.

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* 239894939.

† The sun's mean distance being 1, that of the comet was 2·89797.

‡ The comet's distance from the sun was 2·683196.

§ 275077889. || 9160542.

XI. *On Commerce. Being a second Communication from Mr.* Graham, *in Answer to our Correspondent* Lapis.

To Mr. Tilloch.

Sir,

In my last letter I endeavoured to prove, and illustrate from a variety of articles, that no country could produce all that was necessary for the comfort, health, protection, and security of its inhabitants. I likewise showed, by a reference to very barren and uncomfortable situations, that the inhabitants of such districts, so far from deserting those countries, were rather more attached to the soil than the people of much more favoured climates. Without further recapitulation, I will pass on to another observation of your correspondent Mr. Lapis. I will not call it an objection, because he does not give his opinion as positive; but he is apt to think, from the different languages spoken by the different nations of the world, as well as from the natural barriers of kingdoms, such as large rivers, long ridges of almost impassable mountains, and the still more extended ocean, which rolls its mighty waves between different countries, that the Author of the universe never intended *that* social intercourse between nations which I seemed to argue, but rather that they should be completely independent of each other. I am much pleased with this observation, not so much on account of giving me an opportunity of communicating my ideas on the subject, but as I consider it a matter of great importance, as well as useful and curious disquisition. I hope some of your readers, who have more time and far superior abilities, will favour us with a more full illustration of the subject.

In order to prevent my being misunderstood, it may be here necessary to observe that, in all I have written, I wish to keep in view this idea or leading principle, (viz.) That the Supreme Creator of this world is the universal parent of all its inhabitants; that they are all alike his children; and that all his laws have in themselves a natural tendency to promote the happiness of his creatures. At the same time it is necessary to observe that the benevolent Author of our existence was fully aware of all the weaknesses as well as imperfections of his creatures, and that it was impossible for the whole inhabitants of this earth to be under one government: he has therefore divided or separated different countries and kingdoms by such barriers as I have already mentioned: yet, notwithstanding these great impediments, and what would appear almost insurmountable difficulties, we find that the ambition of man is not fully restrained; for some nations are constantly endeavouring to make inroads on their neighbours: now, if this is the ease under the present formation of this world, surely it would have been much more so if nature had not fixed those strong boundaries. While on the one hand I admit this argument in its fullest latitude, on the other I can never enough admire the kind benevolence of our Creator, in giving such diversity in the productions of the earth to different countries, as to be a very strong incitement for friendly and social intercourse: even the principle of curiosity, which has so powerful an effect on the human mind, is a great inducement to visit distant countries; but this can never be done with either pleasure or satisfaction, unless such nations or people have a friendly communication with each other. I must likewise observe how easily a social intercourse is maintained with all the different parts of the earth when there is no hostile intention;—With what ease and facility can a few merchant-vessels carry the various productions of this country to another, and bring in exchange many articles of which we are in want? But how many difficulties attend the fitting out of an armed fleet! This requires all the united strength and energies of the nation. Thus, while the Supreme Being has laid strong restraints on all the more dangerous passions of men, he at the same time has placed very powerful incitements in the human mind for sociability, and from the diversified productions of the earth has made it their interest to have a friendly intercourse with one another, to behave and act as subjects of one supreme governor, and as the children of one kind and benevolent parent. Were I to call in the aid of the inspired writers, they would appear in full force in favour of my argument; but I only beg leave to recommend a serious perusal of the principles of Christianity to many who assume that name, while their whole conduct is diametrically opposite to its precepts, otherwise we should never have heard of that impious and unchristian maxim, Natural Enemies: from the general idea of the Devil such a supposition would be natural, but to impute such to our merciful Creator is surely horrid impiety.

I hope it will not for a moment be supposed that I here make any allusion to Mr. Lapis,—No, not in the least; but that there are such as I describe is but too evident. As I would not willingly overlook any argument or objection brought forward by Mr. Lapis, I will beg leave to observe, that I fully agree with him in the manner in which he has stated the first beginning or principles of commerce, as belonging to one nation or individual country: but he could not perceive that the same was rendered necessary between different kingdoms. Now I think I have shown in a variety of instances, that there is a greater necessity for an interchange with different countries than with the distinct parts of the same nation; and that, in the present system of the universe, every law or institution contrary to this order of nature may justly be said to counteract the benevolent dispensations of the Creator. The more I consider this subject, the more I am convinced that it is a law or principle which runs through every department of society, from a single individual to that of nations. I am rather surprised that Mr. Lapis should mention (as part of his objection to my

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my statement) that that man who could excel in the manufacture of any article in his own country, had more merit than he who imported it from another:—most certainly he has; and whoever will turn to my first Essay will find, that to promote this spirit amongst the inhabitants I state to be one of the principle objects of a wise and patriotic politician or statesman. I think I have already proved to a demonstration, that there is no country which can produce all that may be said to be of use, comfort, &c., &c., to the inhabitants,—I mean as far as the productions of the soil are concerned. I likewise contend that no exertions, either by individuals or united bodies, can bring the making or manufacture of *every* article to the same degree of perfection to which it is sometimes brought in another country; at least such a phænomenon has not yet appeared in the world, and, for the happiness of the human race, I believe it never will. Nations, like individuals, if they possessed within themselves every requisite which they found necessary or useful, would be too much puffed up with vain-glory, pride, and presumption; would consider themselves as the only favourites of Heaven, and look down with contempt on all their fellow-mortals as beings of an inferior order, and not alike the children of the same kind and benevolent parent. Nations, as well as individuals, with all their wants, weaknesses, and imperfections, are but too apt to fall into this dangerous error:—How much more would it be so if every country did, or even could, by its exertions supply all its wants! Would not such be apt to forget the Creator, and to exclaim, "My own right hand has gotten me the victory?" I am well aware that several foreign articles may justly be termed luxuries, and that a very improper use is often made of them; but if some people will injure their health or hurt their constitution, by excess in that which ought only to be used as a medicine, or for the comfort and support of old age, the blame in all those cases lies with man, who only abuses the bounties of his benevolent Creator. I well know that rice is more congenial to the support of the inhabitants of those countries where it is the natural produce, than it would be to an Englishman who has been accustomed to bread made from flour*. But, even in our years of greatest plenty, is not rice a very useful article? From the general reasonableness of the price, even the lower orders of the people find it pleasant, wholesome, and to them a luxurious treat: only a few years have passed away since all classes of people were happy to find in it a substitute for bread. I am apt to think that the Supreme Governor of the world saw that it was necessary so to constitute the order of nature, that years of scarcity might sometimes occur, to teach ungrateful men the value of his blessings, as well as to show them the necessity of a friendly intercourse with other countries: and I am persuaded that, if that social intercourse for which I contend were more generally adopted, even famine would be divested of half its horrors.

I have already said, that I would neither recommend wine nor foreign brandy, as a common beverage to an English labourer, in preference to good malt liquor; but however preferable this may be while youthful vigour blooms in the countenance, and manly strength braces every nerve: when old age weakens the limbs, bows down the body, and dries up every source of pleasure, Who will deny that a change is often useful, and that even a little wine as well as some other foreign cordial will make glad the heart of man, and occasionally help to cheer the languor of declining years? If this be a true state of the matter, (and I think it will readily be admitted by every person who has made observations on these things,) was I not justified in saying, and now repeating, that it is a cruel policy to deprive the great bulk of the people of any article which may be of such eminent use, and more particularly when nature requires it most? I myself have known several instances where the physician or apothecary have recommended wine: the attending relatives of the patient answered (while the sympathetic tear started from their eyes) "O sir, we have no money, it is too dear, we cannot afford it." But of this I forbear. I will only beg leave to mention one or two more articles, to show the great difficulty, if not utter impossibility, of the people, even in the same country, making some things equal in quality or perfection at a very small distance from each other. London porter is one article:—there is scarcely a town or village of any note in the kingdom, where the making of porter, in imitation of the London, has not been tried. Brewers and malt-makers have been brought from the metropolis at a very great expense, no money has been spared, every effort which human ingenuity could contrive has been exerted,—but all in vain; the difference even to a superficial observer is very evident.

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* It would be superfluous to enter into the disquisition whether the natural produce of every country being more proper for the inhabitants than any foreign substitute, did not proceed more from long habit, than from any positive law or order of nature: for it is a well known fact, that the stomach and constitution of man very soon assimilate to a great variety of food.

I will only mention one more:—the making of Cheshire and Glocester cheese, seemingly a very simple and well known operation; yet I have never seen what could be called a tolerable imitation, and have known great exertions made by some wealthy farmers, both from a principle of emolument and curiosity; but I have never known one who succeeded in any tolerable degree*.

I must now beg your indulgence while I offer a few remarks on the bad policy of high duties. Whether I consider the present system as it affects the revenue, the morals, or the health of the people, it has the most dangerous tendency: it teems with evils of the greatest magnitude. I will not here recapitulate the arguments I used in my first Essay, but will add some others.

To remove as far as possible every alluring incitement to transgress the laws of the country, to place at a distance every temptation which might be supposed too strong for the general virtue of the people, has always been the care and study of every wise legislator, much more than to make severe laws or to inflict cruel punishments. I am fully persuaded that there is no person who has studied human nature, whether by reading or from his own observations in the world, but will admit the wisdom and utility of this maxim.

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* I hope this will not be understood as meaning to damp ardour, or dispirit the exertions of those who wish to persevere in improvements: I only mention the difficulty, without meaning to set bounds to human ingenuity or persevering exertions.

I cannot here omit mentioning the great penetration and humanity of our ancestors in framing many of our laws.

The law is positive, "Thou shalt not steal;" but very great difference is made between the crime of breaking locks and bars to commit theft, and when cash or other valuables are placed in very open exposed situations; and for this very plain and humane reason,—the temptation is supposed to be much more strong in the one case than the other. Compare this with our present system of excise laws, which are of a modern date: in these no allowance is made for the weakness of human nature placed under the strongest temptations, sometimes of poverty; at other times the loss of business, by being undersold by some neighbour who is less scrupulous as to illicit connexions:—even that invaluable privilege, trial by jury, is denied to the great bulk of the people connected with the excise. I have often contemplated with astonishment, that the greatest crimes which can disgrace human creatures are suffered to be tried by a jury the nearest to the place where the crime is connected; but this is denied to every offender against the excise laws, unless he is able, and will submit to the enormous expense, of having the cause tried in the Court of Exchequer at London. I could here adduce a great variety of arguments in support of this proposition. I wish to prove, as well as relate, some circumstances, the unavoidable consequence of the present system, which would astonish some, and excite feelings of pity in the breast of the most obdurate;— but I forbear this.

Lest, however, some may think that I plead too much for the weakness of humanity, I will only beg leave to mention two cases, which, I think, are in point. If the good and pious Agur so earnestly prayed against poverty, lest he should put forth his hands to steal, How necessary then to place, as far as possible, temptation from those whose minds are often little fortified either by piety or morality! If he who well knew the heart of man closed this petition, ("Lead us not into temptation") with those which we are commanded to offer up to our Creator, need I use any further arguments? I must not, however, omit mentioning a well-known fact, which I am afraid is too little attended to by those who fix such enormous duties to certain articles*, viz. There is not one amongst a thousand who considers what is called illicit trade any breach either of religion or morality: if they pay the value of the article to those who sell, they think they have fulfilled every moral obligation. I will now mention a few particulars to show how the health of the people is injured by the present system of high duties. I agree with Mr. Lapis, (and I believe it is generally admitted,) that malt liquor is the most wholesome and best beverage for the great bulk of the people: But, since the present enormous imposts, where can it be obtained genuine? Far be it from me to impeach every brewer of illicit practices; but the public have had sufficient evidence, that, in different instances, various ingredients, and some of a very pernicious nature, besides malt and hops have been made use of. But, supposing no such practices to have been proved, is it not a well-known fact, that not only in breweries, but likewise in distilleries, all the arts of chemistry, and the skill of the most eminent in the profession, are called into action? To produce the colour most likely to please the eye, to obtain the flavour in most general approbation, to cause the liquor to sparkle in the glass, raise a fine head or adhere to the sides of the pot, are, with many other objects, constantly exercising the mind of the operator; and to obtain such on the easiest terms, or at the least expense, his constant aim and study. It is really astonishing how easily some of these objects can be produced by artificial means, which ought only to be the effect of the genuine materials from which the liquor is made. To obtain any foreign liquor in a true genuine state is likewise very difficult and uncertain; for in this the temptation is equally strong. Even allowing that the first importer has strength of mind sufficient against all the allurements of gain, the article goes through so many hands before it reaches general consumption, that all the well known practices of mixing, (particularly when the flavour is strong,) reducing, and again bringing to full proof, are too often carried to a great extent.

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* Taking the first price of tobacco at 5½*d.* per pound, while the duty is 2*s.* 3*d.*, it is evident that one cargo smuggled, yields a fortune to the adventurer.

Even wine is well known to be often so much adulterated, that it is a mixture of no person can tell what. When we consider that this is often used as a medicine, how dangerous and uncertain must the application often be!

Need I recall the attention of your readers to the various means used to adulterate tea before the reduction of the duty? Now it is nearly back to its former high price, the very same consequences will naturally follow*, as soon as any of the countries on the continent shall be in a situation to get tea from China so as it may be smuggled into this country. What with smuggling from abroad, adulteration at home, the high price lessening the consumption, the diminution of the revenue must follow. I should now point out some of the bad effects which high duties have on the morals of the people; but the evils are so numerous, and the consequences so fatal to the peace of society as well as individuals, that the subject would require a separate Essay, and I have already intruded too much on your indulgence. Wishing every success to your useful and entertaining Magazine,

I am, sir, yours, &c.,

JAMES GRAHAM.

Berwick,
Jan. 2, 1809.

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* Before the reduction of the duty on tea took place, the revenue arising from that article had dwindled to a mere trifle, and the same cause will certainly produce the same effect.

XII. *Memoir*

XII. *Memoir upon the Vineyards and Wines of Champagne in France: Written in answer to certain Queries circulated by M.* Chaptal. *By M.* Germon, *of Epernay*[*].

PRELIMINARIES[†].

The ancient province of Champagne, now divided into two departments under the names of La Marne and La Haute-Marne, has been long celebrated as the vineyard of France.

There are two kinds of wines which distinguish this district.

White wines: called *Riviere de Marne* wines.

Red wines: called *Montagne de Rheims* wines.

The white wines are produced from vineyards situated in the valleys and upon the sides of the hills in Epernay, Dizy, Avenay, Cramant, Lemesnil, Monthelon, Chouilly, Moussy, &c.: but in consequence of one of these varieties of nature, for which we cannot always account, the estate of Cumieres, in the midst of so many vineyards celebrated for white wines, and under the same exposure, produces red wines only, and of a quality far superior to the above wines.

Among all the vineyards on the river Marne, the cantons of Hautvillers, Mareuil, Cumieres, and Epernay, are the most advantageously situated: they extend along the river Marne, with this distinction, that the quality of the wine falls off in proportion as the vineyard is distant from the river: for this reason Hautvillers and Ay have always enjoyed a preference over Epernay and Pierry; and the latter over Cramant, Lemesnil, &c., and these last over Monthelon, Moussy, &c.

South exposures produce upon the banks of the Marne excellent white wines, but their declivities and posterior parts, which are called the mountains of Rheims, although situated in general towards the north, and almost always to the east, also give red wines of a good quality, and of a fine taste and aromatic flavour.

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[*] *Annales de Chimie*, vol. lxi. p. 5.

[†] The numerous facts contained in this Memoir render it truly valuable: although the author expresses himself in the language of a good practical cultivator, he does not always display the accuracy of a modern chemist. We have not hitherto met with any thing more comprehensive on the subject; and it forms the materials of M. Chaptal's projected work upon "*L'Art de faire le Vin.*"——Note of the French Editor.

The slope which overhangs Rheims is divided according to the quality of its wines; hence we have wines of the mountain, of the lower mountain, and of the estate St. Thierry.

The mountain comprehends Verzy, St. Basle, Verznay, Mailly, Taissy, Ludes, Chigny, Rilly, and Villers-Allerand; and among these vineyards, the most esteemed are Verzy, Verznay, and Mailly. The rest, although very good, are of a different quality.

The vineyard of Bouzy, which terminates the chain or the horizon between south and east, and which, therefore, belongs to the two divisions, ought not to be omitted. It produces excellent, fine, and delicate red wines, which, from its exposure, participate in the good qualities of Verznay and the good red wines of La Marne.

The lower mountain comprehends a great quantity of vineyard countries; among which we may distinguish Chamery, Ecueil, and Ville Demange: this last place in particular, when the season is good, yields wine which will keep for ten or twelve years.

The lower mountain extends to the banks of the river Aisne. As the wines it produces are of a middling quality, it scarcely requires to be particularized.

The district of Saint Thierry has taken its name, with respect to its wines and vineyards, from a large extent of grounds containing large vineyards, such as Saint Thierry, Trigny, Chenay, Villefranqueux, Douillon, Hermonville, and produce very agreeable red wines of a pale colour, very much in request by the dealers.

But the wine properly called *Clos Saint Thierry*, and coming from the archbishopric of Rheims, is the only wine which unites the rich colour and flavour of Burgundy to the sparkling lightness of Champagne. *Clos Saint Thierry* holds the same rank among Champagne wines, that *Clos-vougeot* does among those of Burgundy.

In the enumeration of the vineyards of the mountain, some readers may perhaps expect to find *Sillery* mentioned, once so remarkable for red and white wines: the truth is, that Sillery wine is in a great measure composed of the wines produced in the territories of Verznay, Mailly, and Saint Basle, once made, by a particular process, by the *marechale d'Estrées*, and for this reason long known by the name of *Vins de la Marechale.* At the revolution this estate was divided, and sold to different rich proprietors of Rheims: the senator of Valencia, however, the heir to a great part of this vineyard, neglects no means of restoring Sillery to its former reputation.

Series of questions put by M. Chaptal, with their answers.

I. *Which is the most advantageous Exposure for the Vine?*

The most advantageous exposure for the vine is, without contradiction, the south and the east; but it has been ascertained that certain advantages of soil and the nature of the plant must also concur: otherwise various districts, such as Damery, Vanteuil, Reuil, &c., with the same exposure and climate, and also watered by the Marne, would enjoy the same celebrity as Cumieres, Hautvillers, and Ay. It must be confessed that the former districts produce inferior kinds of wine; but it remains to be decided whether we ought to ascribe this difference to the culture, the plants, or the soil.

II. *Are the high Exposures, the middle Elevations, or the lower Grounds, best adapted for Vineyards?*

Of all situations, the middle grounds are most esteemed: the heat being more concentrated in them, they are exempt from the variations of the atmosphere which prevail on eminences, and from the humidity and exhalations which issue from the lower regions: the elaboration of the sap or juice is therefore more complete in the middle grounds.

III. *Does an East or West differ much from a South Exposure, in occasioning a sensible Difference in the Quality of the Wines?*

A western exposure is unfavourable to vegetation: it burns and parches without any advantage, nor does it give time for the juice to be elaborated, and spread through all the channels of vegetation, when mists, humidity, or dew, succeed: it is a certain fact, that there is a difference of one third in the quality and value between vines situated in east and west exposures.

IV. *Describe the Nature of the Ground or Soil which produces the best Wine.*

Next to exposure, the nature of the soil and of the ground influences the quality of the wine. It must be admitted, however, that grounds with a northern exposure produce wines of a generous and spirituous description; while another exposure, perhaps to the south, yields a poor and common sort of wine. It is therefore to the salts and the juices of the earth, combined with the influence of the atmosphere, that we must ascribe the goodness and qualities of soils adapted for vineyards.

The most proper soil for vines is a sandy granitic earth, neither compact, nor too thick, nor clayey: frequently in the best exposures, we meet with stony soils, which give very strong wines; but warm and dry seasons are requisite in these cases, and a necessary maturity: beneath these stony soils, there are clayey and unctuous parts, and plenty of springs, which conduce to the elaboration of the juice.

In general throughout Champagne the soils proper for vines rest upon banks of chalk. The vine, indeed, comes up slowly in this kind of soil, but when it has fairly taken root it grows to perfection: the heat of the atmosphere is tempered and modified by the coolness of the chalky beds, the moisture of which is constantly sucked up by the vegetative channels of the vine-plant.

CULTIVATION OF THE VINE.

V. *How is the Vine planted?*

In November or December, when the season admits of it, the vine is planted by making an oblong hole or furrow, one foot and a half in depth, by two or three feet in length: the plant is introduced into it and covered with earth, sloping it in such a way as to uncover only two or three inches of the extremity of the plant, to which a horizontal and erect position is also given. Each hole of this kind is one foot and a half from the one adjoining, and on the same line in vineyards where the soil is rich; two feet being allowed in light soils. An interval of three feet is left between the rows of the plants, and care is taken, when a new row is begun: the plants must not be placed perpendicularly, and directly above each other.

VI. *What is the Way in which the Shoots are made?*

The plants are inserted into turfs, or in *longuettes*. The longuette is a mere naked twig, which had been left the year preceding, and which is now carefully raised and detached, leaving the young roots behind it.

The turf plant, or *marcotte*, consists in digging up a turf in the marshes, and introducing into it in spring, by means of a hole made in the middle of the turf, the *longuette* or slip intended to be planted: this shoot with its earthy appendage is then fixed in the ground, sloping it as usual: the root is formed in the course of the year, and with a pruning-knife the longuette is cut close to the top of the shoot, and they are then removed by men, or on the backs of animals, in order to be afterwards planted: this last way is the most expensive, but it is the surest, and advances the vine very fast in respect to vegetation.

One hundred of longuettes or bare slips cost four or five livres, and turf plants cost from 12 to 14 livres.

But as two longuettes are requisite for each hole or furrow, when they plant in this way there is a trifling saving, although the other method is far preferable.

VII. *Is Grafting advantageous?*

Grafting is not in general use, except in the vines belonging to the vine-dressers themselves, and in the large plant: these vines when grafted become yellow, and languish. The graft remains for some years exposed to the air, humidity, and to bad management of the labourer, and in short to all the intemperance of the climate.

VIII. *How long does a good Vine Plant last?*

A good vine plant lasts 50 or 60 years, and frequently longer, according to the care which has been taken of it.

A vine plant is deteriorated generally by the bad management of the vine-dressers with respect to the shoots or slips: if they are not sunk deep enough in the ground, the vine plant becomes overwhelmed with roots, which at last form a solid cake, and absorb all the juices from the ground: the vine being thus incapable of shooting, the evil ought to be instantly remedied.

IX. *What Kind of Grapes are best adapted for White Wine?*

Black and white grapes are planted indiscriminately in the same vineyard: and this is perhaps wrong; for the term of maturity is not the same with both kinds of grape. The reason assigned for this practice is, that wine made from black grapes alone would be too vinous, and would become muddy (*sujet á tacher*) in hot seasons; while wine made from white grapes would be too soft: the latter kind of grapes would be too soft, as containing more mucilage (*muqueux*).

X. *Is the Black Grape preferable to the White?—State the Cause of this Superiority.*

There is not much variety in the grapes of Champagne.

The black are generally preferred to the white grapes for several reasons: In the first place, the black grapes resist much better the rains and frost so common about vintage time. Secondly, because there is more vinosity and fineness in the black grape, and it gives more of what is called body to the wine: the white on the contrary is too mucilaginous, renders the wine soft, and exposes it to become yellow, or to thicken.

There are whole cantons, however, such as Chouilly, Cramant, Avise, Bisseuil, &c., where there are but very few black grapes, and yet their wine is in high estimation.

XI. *Which of the Exposures is most subject to the Hoar-frosts of Spring?*

The effects of frost are only to be feared at sunrise: the eastern

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eastern exposures are consequently most apt to suffer, although it has been ascertained that vine plants freeze in every exposure.

Thus, all the preservative methods hitherto indicated, such as fumigations, or poles armed with long branches of foliage capable of being agitated by the air, are mere reveries of the imagination: they have been employed indeed in small enclosures; but they never preserved a single cluster of grapes, and are incapable of being applied to a large vineyard.

XII. *At what Period is the Vine to be pruned?*

About the end of February or beginning of March, the most essential operation must be performed, namely, that of cutting the plant. When it is very strong, two branches or stumps only are left.

XIII. *How many Eyes are left in the Plant?*

Three eyes upon each branch: when the vine is weak, one branch only is cut off.

XIV. *At what Height from the Ground is the Plant pruned?*

When the plant is young and the rind is not marked with old prunings, the plant is cut at the height of three or four inches: the vine-dressers cut higher, because they frequently cultivate three branches, and leave four eyes.

XV. *To what Height is the Vine allowed to rise?*

Not higher than a foot and a half,—to avoid dilating the sap too much.

XVI. *At what Season does the first Operation in the Vineyards commence?*

After having pruned the vine, the first occupation is that of hoeing: this operation consists in digging up the earth around the plants, so as to uncover their roots for a moment, and detach the earth from them which may have become clotted; the hoe being always inserted into the earth about a foot from the plant.

At the end of March, or beginning of April, when the thaws have softened the ground, the hoeing commences.

XVII. *What is the Period of Planting by Slips or Cuttinsg?*

This kind of planting is performed at the time when the vine is planted.

XVIII. *In what Manner is this Kind of Planting managed?*

In pruning, the vine-dresser reserves, in the barest and most sterile places, certain slips, upon which he leaves only two or three stalks, according to the strength of the slip: the hole or furrow being made, the slip is gently inclined, by disengaging the roots, and by means of a pair of tongs the stalks are held while placing in the furrow, at from four to six inches distance from each other: the slip being thus fixed at the depth of a foot or thereabout, a hand-basketfull of manure is thrown at the root of the slip; the hole is then filled up with natural earth in a loose manner, in order to admit of the two or three stalks sending out their shoots without being bruised.

XIX. *How many Operations are there to be performed between the Pruning and the Vintage Season?*

The prunings being over, as the same vines are not pruned every year, and even in those which have been pruned the earth has not been thoroughly stirred, the vines are trimmed at the beginning of May: this trimming is called *labourage au bourgeon*, and is followed by the tyeing up of the vine plants.

XX. *Which is the most favourable Moment for Tyeing and Paring the Vine?*

While the vine is in flower, it must not be touched: it must be pared when the flower has nearly passed away, and at the height indicated in Art. XV.: it must afterwards be tied in such a way as to envelop the slip, without injuring the circulation of the air or the growth of the suckers.

Finally; about the middle of August, in order to clear away the grass from the roots of the plant, and to raise up the grapes which may have fallen to the ground, a third and last trimming takes place.

The following is the routine practised in the vineyards of Champagne:

1. They are cut in February or March.
2. Hoed in March.
3. Pruned in April and May.
4. Tied or propped up in April and May.
5. First trimming for the shoots.
6. Pare and tie in June.
7. Second trimming in July.
8. Third trimming in August.

XXI. *How is it ascertained that the Grape is sufficiently ripe, in order to commence the Labours of the Vintage?*

At the end of September, or later if the season has been backward,—before proceeding to the labours of the vintage, in order to obtain the fruit at the most complete state of ripeness,

The stalk of the grape must be brown and woody;

The grape pendent;

The skin or pellicle of the grape tender, and not brittle when chewed;

When a seed can be easily detached from the juice of the grape: which should in its turn present a vinous and transparent appearance, without having any green in it;

When the grape stones are brown, dry, and not glutinous.

OF THE VINTAGE.

XXII. *What Precautions are necessary for managing the Grapes so as not to injure the White Wines?*

Many precautions, even of detail, are necessary in making white wine.

These consist in carefully picking the ripest and soundest grapes from all withered or bruised grapes: they are then put into panniers, and covered with cloths to prevent the effects of the sun's rays, and to avoid fermentation.

The panniers thus covered, being put upon the backs of horses, are conveyed to the press; into which they are not emptied, however, until after sun-set. From twenty to forty panniers full are put under the press at a time: the contents of two panniers produce half a piece of wine: forty panniers yield nine or ten pieces of white wine, and each piece contains two hundred bottles.

[To be continued.]

XIII. *Mr.*

XIII. *Mr. Davy's Theory.*

To Mr. Tilloch.

SIR,

"I THANK you for your early insertion of my former communication, as by that means I was favoured by Mr. Davy's observations on it in his lecture yesterday.

"It seems that I misunderstood him: it is fit therefore that I should state that I did so. He did not assert (in reference to the experiment of the decomposition of the sulphate of potash) that the sulphuric acid and the potash repelled each other in consequence of being in opposite states of electricity, but in consequence of being brought into the same state.

"I had understood him to say that the decomposition took place in consequence of the natural electricities of the sulphuric acid and the potash being reversed by means of the Galvanic apparatus; and I was less disposed to suspect that I was wrong, from observing the following passage in the Bakerian Lecture for 1807, which seemed to me to convey the same doctrine: 'In the decompositions and changes presented by the effects of electricity, the different bodies naturally possessed of chemical affinities appear incapable of combining, or of remaining in combination, when placed in a state of electricity different from their natural order.' Philosophical Transactions, 1807, p. 38; and as, in the experiment alluded to, both the sulphuric acid and potash seemed to me to be placed in a state of electricity different from their natural one, I was by this means confirmed in my mistake.

"That I did misunderstand him, however, I am amply satisfied by Mr. Davy's declaration, which is moreover shown by the following passage, which, had it occurred to me at the time, would probably have pointed out my error: it refers to the theory of the decomposition of the fixed alkalies, and is as follows: 'The oxygen being naturally possessed of the negative energy, and the basis of the positive, do not remain in combination when either of them is brought into an electrical state opposite to its natural one.' Phil. Trans. 1808, p. 9.

"Hence, therefore, we are to understand, that in the experiment of the sulphat of potash to which I have so often referred,

ferred, upon this neutral salt being placed in the Galvanic circle, either the sulphuric acid or the potash, one or the other of them, is brought into an electrical state opposite to its natural one; consequently, into the same state as the body with which it was united: hence a repulsion ensues, and the compound body is decomposed. This is Mr. Davy's explanation.

"But I would still presume to request that my former proposed explanation may be considered (which is not in the least affected by my having misunderstood Mr. Davy's explanation); for the difficulties in the way of the present seem to be, that we must suppose that in the first instance one only of the parts of the compound body is affected by the battery, and has its natural state of electricity reversed; and that, ultimately, both are affected; for, the experiment being completed, the acid is found to be positive, and the potash negative.

"Is there, however, any *repulsion* in the case? Should we not rather consider it as a case of chemical decomposition, in which the attraction between the two constituents of the salt is overcome by more powerful affinities?

I remain your obliged humble servant,

January 8, 1809. AUDITOR.

XIV. *Mr.* DAVY'S *Theory.*

To Mr. Tilloch.

London,
January 25, 1809.

SIR,

YOUR correspondent 'Auditor,' notwithstanding his penetration, has, I think, misunderstood Professor Davy's reasoning on electrochemical attraction. His remarks on the inconsistency of the theory and its explanation, such as he conceived them to be, must naturally occur to every considerate reader.

But if my memory does not deceive me, Mr. Davy stated: "An acid that is artificially rendered positive will not combine with an alkali that is naturally positive, and *vice versâ.*" Hence it is evident that, if the electrical state of either the acid or alkali in the neutral salt is changed, they can no longer remain in union, as they instantly exert towards each other a repulsive power proportionable to the inverse energy of their natural electrical affinity. This principle of decomposition, which Mr. Davy merely mentioned in the lecture referred to by Auditor, was fully explained and illustrated in a subsequent one.

He showed, by refined applications of his principles, that in the decomposition of a neutral salt in solution the order of the arrangement varies. When copper wires, which readily combine with oxygen, and are easily soluble in an acid, are used to transmit the electricity, the positive wire attracts the oxygen and acid, and repels the hydrogen and alkali. But when platina wires are employed, which have but a very slight affinity for oxygen and acid, the phænomenon is very different. Oxygen and acid, as before, are attracted by the positive pole; but as they are incapable of uniting with the platina, they instantly receive by contact its electric state, and exercise a repulsive power towards it: the same effect takes place with the hydrogen and alkali at the negative pole.

If we follow this course of reasoning, it is evident that the gaseous oxygen and hydrogen must diffuse themselves in the atmosphere as they are liberated from their combinations, and the acid and potash must find their states of rest at a little distance from the positive and negative poles.

I am, sir, with great respect,
Your humble servant,
A. B.

XV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

JANUARY 12.—The conclusion of Mr. Davy's Bakerian Lecture was read. In this part of his communication Mr. Davy gave an account of the decomposition of the fluoric acid; detailed some curious experiments upon the muriatic acid;

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acid; and entered into various new views connected with chemical theory.

Potassium burns, as Mr. Davy discovered, in fluoric acid gas, and separates its *basis*, which combines with the potash formed, or with the potassium, if this last be in excess; and this compound of the fluoric basis, and the alkali or alkaline basis, produces fluate of potash by combustion, or by the action of water.

Common muriatic acid gas, Mr. Davy has discovered, contains at least a third of its weight of water. Mr. D. has not been able to procure it free from water in an uncombined state; but he has obtained combinations of muriatic acid with phosphorous acid, phosphoric acid, sulphuric acid, and with phosphorus, free from moisture; and these compounds, even when fluid, though constituted by matter supposed to be intensely acid, do not act on litmus paper nor dissolve alkalis, and are non-conductors of electricity; but a very small quantity of water develops their energies, renders them conductors, and makes them capable of violently acting upon litmus and alkaline bodies. With these compounds of muriatic acid, potassium detonates violently even at common temperatures. The energy of the explosion has hitherto prevented Mr. Davy from examining the results; but he thinks it probable that the muriatic acid may undergo change or decomposition in the experiment.

In the course of his general inquiries Mr. Davy examined an experiment, (lately published in Mr. Nicholson's Journal) on the production of ammonia, from a pyrophorus moistened with water, by Professor Woodhouse; and states that he has found his results accurate; but that the formation of the volatile alkali depends upon nitrogen absorbed from the atmosphere by the charcoal employed.

Mr. Davy, by exposing the pyrophorus whilst cooling to hydrogen gas, found that no ammonia could then be produced by the affusion of water.

Mr. Davy, from experiments made upon a large scale, confirms his former analysis of potash, as consisting of about 14 of oxygen to 86 of metal.

He defends the theory of Lavoisier against the opinions of some of the disciples of this illustrious man, who suppose the metals to be compounds of hydrogen.

Jan. 19 and 26.—A. Marsden, esq., Vice-president, in the chair. Part of a long paper, illustrated with several drawings by Mr. Troughton, mathematical instrument maker, was read, describing his instruments and methods of graduating quadrants, sections of circles, and other instruments for mathematical and philosophical experiments. Mr. Troughton's theoretical method consists in making out a table of errors, by which means he corrects the dots made on the graduated circle, previous to the application of his instrument for dividing it into 180 degrees. Of this instrument, invented by his brother, and improved by himself, no correct idea can be given without the drawings, which unfold the whole secret of the author's superior mode of manufacturing mathematical instruments.

WERNERIAN NATURAL HISTORY SOCIETY.

At the meeting of this Society on the 14th of January, Dr. Thomas Thomson read an interesting description and analysis of a particular variety of copper-glance from North America.

At the same meeting Dr. John Barclay communicated some highly curious observations which he had made on the caudal vertebræ of the Great Sea Snake, (formerly mentioned) which exhibit in their structure some beautiful provisions of Nature, not hitherto observed in the vertebræ of any other animal.

And Mr. Patrick Neill read an ample and interesting account of this new animal, collected from different sources, especially letters of undoubted authority, which he had received from the Orkneys. He stated, however, that owing to the tempestuous season, the head, fin, sternum, and dorsal vertebræ, promised some weeks ago to the University Museum of Edinburgh, had not yet arrived; but that he had received a note from Gilbert Meason, esq., (the gentleman on whose estate in Stronsa the sea snake was cast,) intimating that they might be expected by the earliest arrivals from Orkney. In the mean time, he submitted to the

Society the first sketch of a generic character. The name proposed for this new genus was *Halsydrus*, (from ἁλς the sea, and ὕδρος a water-snake); and as it evidently appeared to be the Soe-Ormen described above half a century ago, by Pontoppidan, in his Natural History of Norway, it was suggested that its specific name should be *H. Pontoppidani*.

XVI. *Intelligence and Miscellaneous Articles.*

EARTHQUAKE.

THE following account of a shock of an earthquake felt at Dunning, in Perthshire, on the 18th of January, about two o'clock A. M. is given by Mr. Peter Martin, surgeon, in Dunning.—He was coming home at the time on horseback, when his attention was suddenly attracted by a seemingly subterraneous noise, and his horse immediately stopping, he perceived the sound to proceed from the north-west. After continuing the space of half a minute, it became louder and louder, and apparently nearer, when, all on a sudden, the earth gave a perpendicular heave, and with a tremulous waving motion seemed to roll or move in a south-east direction. The noise was greater during the shock than before it, and for some seconds after it was so loud, that it made the circumjacent mountains reecho with the sound; after which, in the course of about half a minute, it gradually died away. At this time the atmosphere was calm, dense, and cloudy, and for some hours before and after there was not the least motion in the air. Fahrenheit's thermometer, when examined (about half an hour after the shock), indicated a temperature of 15 degrees below the freezing point of water. The preceding day was calm and cloudy; thermometer, eight A. M. 14, eight P. M. 13. The morning of the 18th was calm and cloudy, but the day broke up to sunshine; thermometer, eight A. M. 19, eight P. M. 16. This was a greater shock than that felt at the same place on the 9th of September, about six A. M. several years ago; and if it had been succeeded by another equally violent, it must have damaged the houses: but fortunately we have heard of no harm being done.

NATURAL

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NATURAL HISTORY.—At day-break on the 3d of January 1809, an enormous fish was descried at half cable's length from Penryn quay, steering towards the town, and three boats, under the direction of captain Dunn, were manned to attack him: the first he enclosed, as it were in a pond, formed by a circular curve from head to tail, without doing any injury. A man then courageously cut a hole in the dorsal fin, through which he rove a hooked rope. Upon feeling this, the fish attempted to put to sea, but being diverted by some hard blows on his snout, he sheered towards the Falmouth road. A three-inch rope doubled was then parbuckled round him, which he instantaneously snapped. A hawser from the quay was next applied to him; when, after dragging a sloop's anchor, tearing up a moorstone post on the quay, and staving a boat, he was brought into shoal water, and, it being ebb tide, subdued. He was afterwards towed round by three boats, and with the tackle of a sand-barge and the exertions of 20 men and three horses, he was drawn upon the slip of colonel Heame's quay, where he remained a few days for the amusement of the curious. He measures 31 feet long, 19 feet round, 9½ feet high, 7½ feet mouth. It proved to be a male of the Squalus genus, being the Squalus maximus, the Basking Shark, or Sun-fish of Pennant. It abounds in the Irish Channel and on the west coast of Scotland. It is generally seen in pairs. Accordingly the mate of this animal was observed in St. Keverne Bay, next day, by the Walsingham packet.

Mr. Taylor the Platonist announces, that he has made some very important discoveries in that branch of the mathematics relating to infinitesimals and infinite series. One of these discoveries consists in the ability of ascertaining the last term of a great variety of infinite series, whether such series are composed of whole numbers or fractions. Mr. Taylor further announces, as the result of these discoveries, that he is able to demonstrate, that all the leading propositions in Dr. Wallis's Arithmetic of Infinites are false; and that the doctrine of Fluxions is founded on false principles, and, as well as the Arithmetic of Infinites, is a most remarkable instance of the possibility of deducing true conclusions from erroneous principles. Mr. T. is now composing a treatise on this subject, which will be published in the course of next year.

LECTURES.

St. Thomas's and Guy's Hospitals.

The Spring Course of Lectures at these contiguous Hospitals will commence as usual the 1st of February, viz.

At St. Thomas's. Anatomy and Operations of Surgery, by Mr. Cline, and Mr. Cooper.—Principles and Practice of Surgery, by Mr. Cooper.

At Guy's. Practice of Medicine, by Dr. Babington and Dr. Curry.—Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental Philosophy, by Mr. Allen.—Theory of Medicine, and Materia Medica, by Dr. Curry and Dr. Cholmeley.—Midwifery, and Diseases of Women and Children, by Dr. Haighton.—Physiology, or Laws of the Animal Œconomy, by Dr. Haighton.—Occasional Clinical Lectures on Select Medical Cases, by Dr. Babington, Dr. Curry, and Dr. Marcet.—Structure and Diseases of the Teeth, by Mr. Fox.

N. B. These sveral Lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a complete Course of Medical and Chirurgical Instructions. Terms and other Particulars may be learnt at the respective hospitals.

Mr. Singer's extensive Course of Lectures on Electricity will commence at the Scientific Institution, 3, Princes-Street, Cavendish-Square, about the middle of February. They comprise a Historical View of the Progress of Electrical Discovery, from the earliest period to the present time; and an Exhibition of every interesting Experiment, with their Application to the Solution of Natural Phænomena, and to the Purposes of Philosophical Research: assisted by Original Illustrations on an Apparatus of considerable extent and power.

LIST OF PATENTS FOR NEW INVENTIONS.

To Phineas Andrews, of Haverstock Hill, in the parish of Hampstead, in the county of Middlesex, gent., for certain

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certain improvements in the construction of a machine for thrashing of corn, grain, and pulse, and all kinds of seed. October 31.

To Samuel Crackles, of Kingston-upon-Hull, brush-manufacturer, for a method of making brushes from whalebone, formerly made from bristles. November 3.

To Samuel Brookes, of Bermondsey, tanner, for an invention for splitting raw bull, ox, and cow hides, so that each side of the hide so split, may be manufactured for the purposes for which an entire hide has been before used—as follows: the grain side for coach and chaise hides and other purposes, and the flesh side for losh hides, for white leather, for vellum, for tanning, and for other purposes. Nov. 8.

To John Hartley, John Musgrave, and William Farmery, of Leeds, machine makers, for a machine for preparing roving, slubbing, spinning, twisting, and doubling of cotton, flax, hemp, tow, worsted, silk, or any other substance, into threads, preparatory to their being manufactured or otherwise used. Nov. 8.

To Nicholas Fairles, of South Shields, esq., for a windlass, windlass bitts, and metallic hawse hole chamber, whereby great manual labour is saved, and a less space of time is necessary in heaving-to and getting on board ships' anchors, either in moderate weather or in gales of wind. Nov. 15.

To Jonathan Dickson, of Christ-Church, Surrey, steam-engine maker, for improvements in the construction of tuns, coolers, vats, and backs, used by brewers, distillers, and others. Nov. 15.

To Charles Gostling Townley, of Ramsgate, esq., for improvements applicable to musical instruments of different descriptions. Nov. 26.

To Frederick Nolan, of Stratford, near Colchester, in the county of Essex, clerk, for improvements in the construction of flutes, flageolets, hautboys, and other wind instruments now in use. Nov. 26.

To Charles Seward, of Lancaster, block-tin manufacturer, for improvements in the construction of lamps. Nov. 26.

To John Schmidt, of Saint Mary Axe, London, watchmaker, for a phantasmagoric chronometer or nocturnal dial, representing or making visible at night, to an enlarged size, the dial of a watch against the wall of a room; the reflection obtained by a light and optical apparatus being at the same time sufficient to give the room a pleasing illumination. The nocturnal dial may, with little alteration, be constructed of any watch or time-piece: but to render the whole as simple and useful as possible, he has also invented a mechanism, or instrument, which is applicable to the above, on account of its peculiar action, which he calls the mysterious circulator, or chronological equilibrium, requiring only one hand or nonius to show seconds, minutes, and hours: it is particularly useful, and may, if required, with little alteration, represent an orrery. Dec. 20.

To John Frederick Archbold, of Great Charlotte-Street, Blackfriars-Road, for improvements in making brandy; comprising, first, a new method in making wine as the worts or must for the making the brandy, and a still applicable to the working off the same, and a new method of rectifying the spirit when worked off. Dec. 20.

To William Steel, of Liverpool, glass-dealer, for an entire new machine engine, or instrument for making white salt. Dec. 29.

To William Tompson, of Dent End, near Birmingham, in the county of Warwick, locksmith, for a lock, which acts in a perpendicular and horizontal direction, with spring and tumblers, one part being at liberty whilst the other is in motion, the bolts of which lock return into the body thereof when it is unlocked. Dec. 29.

METEOROLOGY.

The weather during the present month has been the most severe that has been remembered for many years. The fall of snow has been excessive, and the degree of cold very intense. In Scotland the weather has been similar. At Edinburgh on Saturday the 21st of January, at eleven o'clock P. M. a thermometer constructed by Crighton of Glasgow, stood at 17°. On the following morning, at half past eight o'clock, it stood at 12°; and at eleven at night it was so low as 9°. On the 23d in the morning the temperature was 17°.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CAREY, OF THE STRAND,

For January 1809.

Days of the Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Dec.	27	30°	35°	36°	29·52	0	Rain
	28	37	38	37	·50	0	Rain
	29	38	42	39	·49	4	Cloudy
	30	40	45	39	·50	4	Cloudy
	31	39	37	37	·58	0	Small Rain
Jan.	1	38	38	38	·57	0	Rain
	2	38	40	33	·42	5	Cloudy
	3	31	32	30	·35	0	Snow
	4	30	33	32	·65	0	Small Rain
	5	33	33	33	·65	5	Cloudy
	6	39	46	44	·50	4	Cloudy
	7	44	44	42	·15	0	Rain
	8	44	44	41	28·50	0	Rain
	9	41	43	42	29·20	0	Rain
	10	43	46	37	·14	8	Cloudy
	11	38	45	41	·40	8	Fair
	12	38	40	35	·50	12	Cloudy
	13	34	38	37	·68	0	Rain
	14	31	33	30	·80	16	Fair
	15	28	29	29	·75	0	Snow
	16	28	30	27	30·05	25	Cloudy
	17	28	28	25	·01	27	Fair
	18	21	26	22	29·85	25	Cloudy
	19	22	28	31	·58	0	Cloudy. In the afternoon there was a storm of rain and sleet.
	20	31	32	32	·44	0	Cloudy
	21	32	34	33	·50	0	Cloudy
	22	32	34	33	·04	0	Snow
	23	22	30	31	·70	4	Fair
	24	33	35	40	·45	0	Rain
	25	42	36	37	·75	24	Cloudy
	26	42	48	45	·20	0	Stormy

N. B. The Barometer's height is taken at one o'clock.

XVII. *On Barometrical Measurements*[*].

To Mr. Tilloch,—Sir,

M. De la Place in his *Méchanique Céleste*, M. Ramond in the *Mémoires de l'Institut*, vol. vi., and M. Daubisson in the *Journal des Mines*, February 1807, have recently brought to perfection, the method of measuring altitudes by means of the barometers. Their memoirs are highly deserving of being translated into English: it is at least certain, however, that it would be agreeable to the geologists of this country, and to those of foreign nations who make use of instruments graduated in the English way, if the second of the two methods of M. Ramond was reduced into the measurements of this country. This second method has the advantages over the first of being the most expeditious, and at the same time almost imperceptibly exact. I have undertaken this task, and shall be happy to see it inserted in your Journal, if you find it worthy of publication.

The principal part of this work consisted in the reduction of the table of M. Ramond; the object of which is, to have a view in calculation, to the gravity at different parallels of latitude at which we take observations. His second method abridges considerably his first; and, in my opinion, all antecedent methods where exactitude was the object in view. I subjoin this table calculated upon the co-efficient 10057·6 fathoms, which corresponds to 18393 metres, being that of M. Ramond.

You will perceive that the result of the calculation, according to my reduction, gives the altitudes in fathoms in place of giving them in feet as in England. In this trifling change, I find two advantages: In the first place it renders the calculation a little shorter; and secondly, it presents the altitudes in numbers more easily fixed in the imagination than more complicated numbers. In short, we cannot form a very distinct idea of the height of a mountain, when we state it as 19000 feet, whereas three miles and some fathoms will impress the real height much more strongly on the mind.

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[*] For this communication we are indebted to a learned foreigner now in London.

The examples subjoined show the method; and I presume will require no further illustration to those who are acquainted with logarithms. I shall only observe, that in reducing the metre to fathoms, I have made use of the report which I found in the Memoirs of the Royal Institution, vol. i., namely, $1^m39{\cdot}371$ English inches: and with respect to the thermometer, I have made use of the well known report 100 : 180, those two numbers marking the space of the scale comprehended between the freezing and boiling points: thus in calculation the degrees of Fahrenheit should be diminished 32° for the degrees above the freezing point, and on the contrary with respect to the degrees below melting ice, they must be retrenched from 32°.

I am your very obedient servant,

De J——.

January 1809.

Note—In the first of the following examples I have applied to the least height of the barometer, the difference of the thermometers which are attached to them, because the thermometer was lower than in the other.

In the second example it is the contrary, because the thermometer attached was highest at the station where the barometer was lowest. The rule is to augment the height of the column of mercury, in the coldest station, by so much as $\frac{1}{9741\cdot 6}$ as there are degrees of difference between the thermometers of correction. The perusal of M. Ramond's Memoir will greatly assist the reader on this subject.

Example

Example I.

	Barometer.		*Fahrenheit's Thermom. fixed.*	*Fahrenheit's Thermom. free.*
At Tarbes*	28·961	English inches	(65°·51) — 32° = 33°·51.	(66·41) — 32° = 34°·41.
Pic du Midi	21·150	- - -	(49·55) — 32 = 17·55.	(39·20) — 32 = 7·20.
Latitude	43°.		Difference 15·96	41·61
				Two Sums 83·22

28·961 - - - - - - - Log. 1·4698069.
21·150 - - - - Log. 1·3253148.
9741·6 Constant + diff. 15·9
= 9767·6 - - - Log. 3·9893430.
9741·6 Constant - - Log. 6·0113697.

1·3260275 1·3260275.

Difference between the logarithms of the altitudes 0·1437794 Log. 9·1328338.

Take in the subjoined Table for 43° of latitude, the logarithm 4·0025805.

$$\frac{1800 \text{ Constant} + \text{two sums } 83{\cdot}22}{1800} = \frac{1883{\cdot}2}{1800} = 1{\cdot}0462. \quad \text{Log. } 0{\cdot}0196147.$$

Difference of the heights in fathoms 1429. 3·1550290

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* Tarbes is a town in the Pyrenees, (province of Bigorre,) which lies at the foot of the mountain called *Pic du Midi*.

Example II.

	Barometer.	*Fahrenheit's Thermom. fixed.*	*Fahrenheit's Thermom. free.*
Summit of mount Saleve *	28·3951	(72·1) — 32 = 40·°1	(73·9) — 32 = 41·9
Foot of ditto - -	25·7120	(78·) — 32 = 46·	(65·) — 32 = 33·
Latitude	46.°10′	Difference 5·9	74·9
			Two Sums 149·8

25·712 - - - - - -			Log. 1·4101359	
28·3951 - -	Log. 1·4532434			
9741·6 + 5·9 = 9747·5	Log. 3·9888932			
Constant -	Log. 6·0113697			
	1·4535063		1·4535063	
			0·0433704	Log. 8·6371934
By the Table for 46° of latitude nearly - -				4·0024513
$\frac{1800 + 149 \cdot 8}{1800} = \frac{1949 \cdot 8}{1800} = 1 \cdot 0833$ - - - - -				Log. 0·0347487
Difference of the altitudes, fathoms 472·49				2·6743934

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* Mount Saleve is in Switzerland.

Table of the Logarithms of the Coefficients calculated for different Latitudes—Supposing the coefficient equal to 10057·6 fathoms for the 45 degrees.

Latitude.	Logarithms.	Latitude.	Logarithms.
1°	4·0037276	37°	4·0028344
2	7254	38	7930
3	7215	39	7510
4	7164	40	7087
5	7097	41	6662
6	7014	42	6234
7	6917	43	5805
8	6816	44	5375
9	6683	45	4944
10	6540	46	4513
11	6385	47	4083
12	6217	48	3654
13	6035	49	3226
14	5839	50	2801
15	5630	51	2378
16	5609	52	1958
17	5174	53	1544
18	4927	54	1131
19	4668	55	0723
20	4397	56	0321
21	4114	57	4·0019924
22	3820	58	9534
23	3516	59	9194
24	3201	60	8774
25	2876	61	8404
26	2541	62	7048
27	2197	63	7691
28	1845	64	7347
29	1484	65	7012
30	1114	66	6687
31	0737	67	6372
32	0354	68	6068
33	4·0029964	69	5774
34	9567	70	5491
35	9165	71	5220
36	8757	90	2604

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XVIII. *A Letter on the Alterations that have taken place in the Structure of Rocks, on the Surface of the basaltic Country in the Counties of Derry and Antrim. Addressed to* HUMPHRY DAVY, *Esq., Sec. R. S. By* WILLIAM RICHARDSON, *D.D.* *

SIR,

I REQUEST you will be so good as to lay before the Royal Society the following observations on the Natural History of that part of Antrim, (contiguous to the Giant's Causeway,) which you and I examined so carefully together. I know not any country that deserves so well to have its facts faithfully recorded; from the important conclusions to which they lead.

The basaltic area (taken in its whole extent) comprehends the greater part of Antrim, and the east side of Derry to a considerable depth.

In a geological point of view, nature † has been very kind to this district, for not content with assembling together in a small space so many of her curious productions, and arranging them with more regularity and steadiness than in any other country described, she has condescended occasionally to withdraw the veil, and lay herself open to view, often exhibiting a spectacle equally gratifying to the admirer of magnificence, and to the curious naturalist, who can here, by simple inspection, trace the arrangements which are to be discovered elsewhere, only by penetrating beneath the surface of the earth.

As soon as we enter the basaltic area, we begin to perceive traces of these arrangements; as we advance further north, they increase; and in the tract near the shore, and especially at the island of *Rathlin*, which seems to have come fresher from the hand of nature than the rest of our area, the stratification of the whole is perfectly visible, and the

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* From Philosophical Transactions for 1808.

† By the word nature, which frequently occurs in the course of this Memoir, I always mean, according to Ray's definition, the wisdom of God in the creation of the world.

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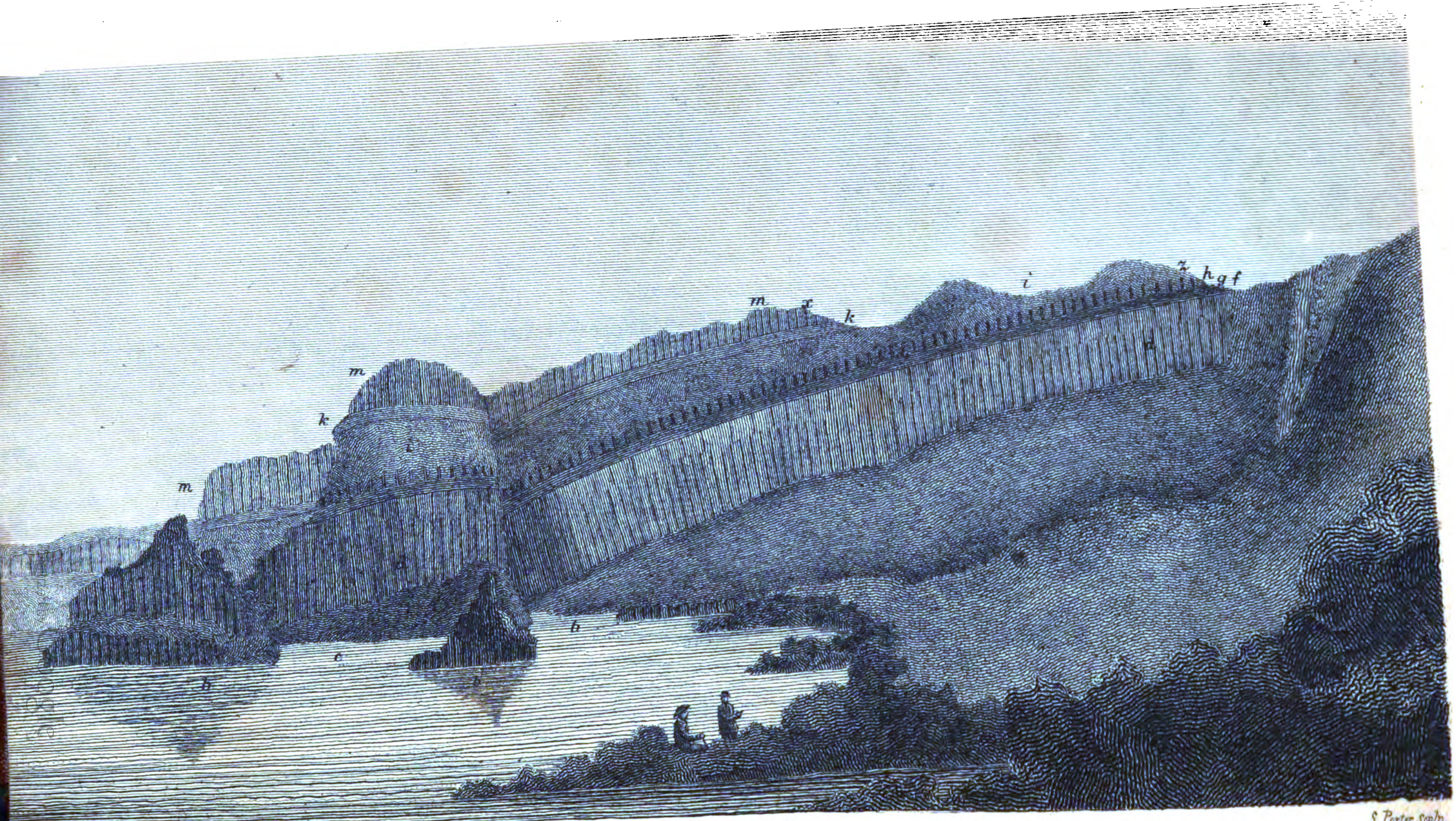

View of Portmoon.

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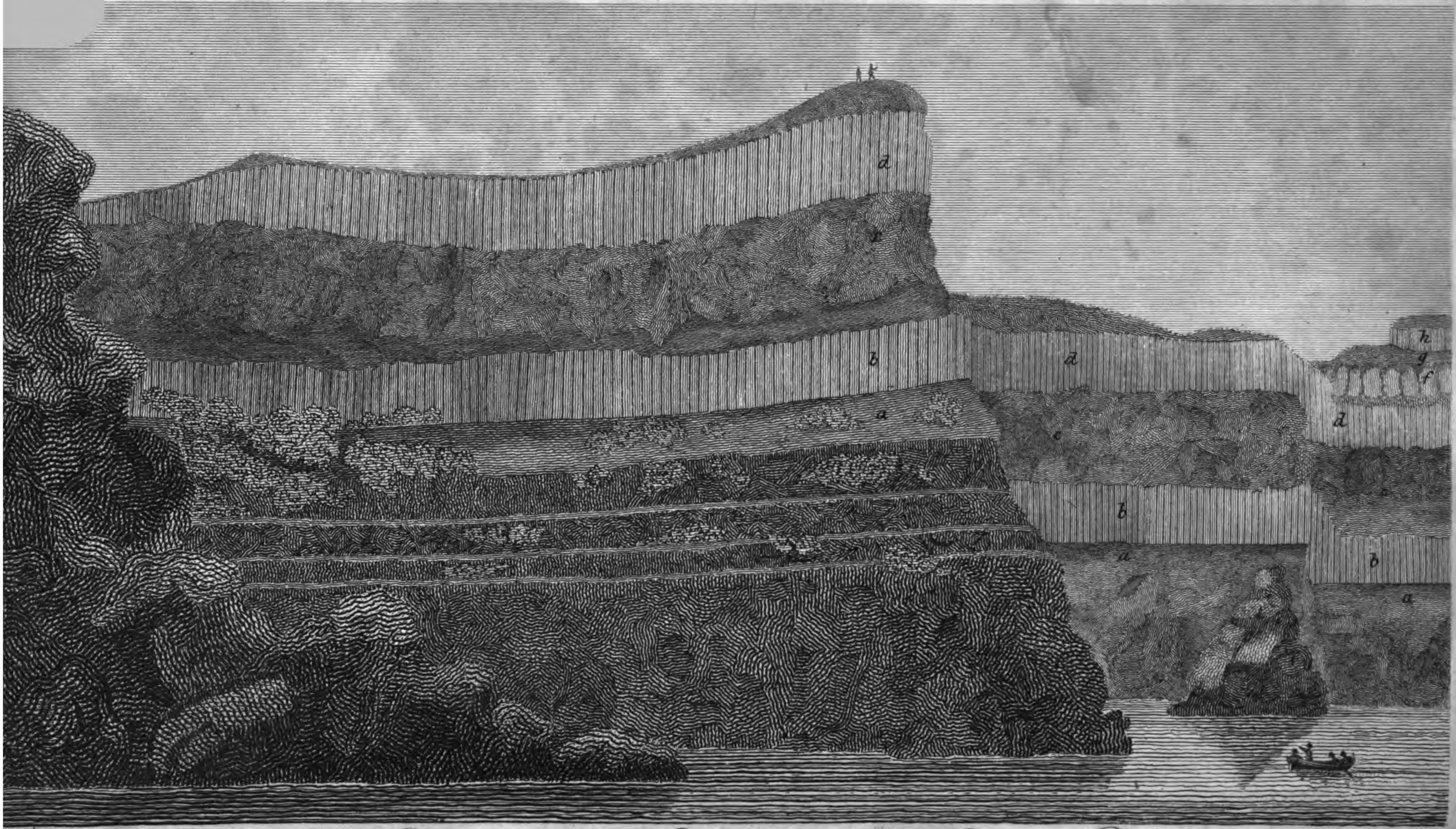
Phil. Mag. Vol. XXXIII. Pl. IV.



S. Porter, sculp.

View of Pleskin on the North West side of Bengore Promontory.

the nature of the several strata laid open to us at their abrupt and precipitous terminations.

To the southward we perceive the distinctive features abate, and wear away; the basaltic stratification indeed remains, but is no longer displayed to us in the same manner; the neat, prismatic, internal construction of the strata, which occurs so frequently on, and near, the coast, is scarcely to be met with at a distance from it; a rude columnar appearance is all we find, and that but rarely.

It is at the periphery of our area, and especially at its northern side, that every thing is displayed to the greatest advantage; here we have perpendicular façades often continuous for miles; and every separate stratum completely open to examination.

Of these façades, four are more distinguished by their grandeur and beauty than the rest, *Magilligan Rock, Cave Hill, Bengore,* and *Fairhead.*

The two former are at the extreme points of the north-west diagonal of our area, and nearly forty miles asunder; they are at the summits of mountains, and accessible by land.

The precipitous faces of *Fairhead* and *Bengore,* to which I had the pleasure of attending you, and which are visible only from the sea, are the most beautiful, and the most curious; for the strata, which at *Magilligan* and *Cave Hill,* are all nearly similar, at *Fairhead* and *Bengore* are much diversified. Of *Fairhead* I have already published an account in Nicholson's Journal for December, 1801, and I now propose to execute an intention which I have had for some years of giving a minute account of *Bengore.*

I am aware that it will be extremely difficult to convey a clear and adequate idea of an assemblage of 16 strata, (for such is the number of which our promontory is composed,) appearing and disappearing at various altitudes, yet retaining each its own proper place, and forming together a most beautiful and regular whole, though never considered as such before.

But as I have the aid of very correct views of the most important parts of the façade, to the accuracy and fidelity of which I have already obtained your testimony—I shall venture to proceed, for I am anxious to bring into notice the most complete exposure of the internal structure of a district, that I have seen or read of; as there is little likelihood that any other person will enjoy the opportunities which I have had for so many years, of exploring this interesting part of our coast, through a turbulent sea, and rapid tides.

Description of the Promontory of Bengore, and its Stratification.

This promontory commences at the termination of *Bushfoot Strand*, where the coast, the general direction of which for several miles had been due east and west, turns to the north-east, and, after being cut into several semi-circular bays, deflects to the S.S.E. and near the old castle of *Dunseverick*, resumes its former rectilineal and nearly eastern direction.

The promontory occupies the interval between *Dunseverick*, and the *Black Rock*, at the end of *Bushfoot Strand*, about four English miles; the façades commence at *Black Rock*, and increase in height until we reach *Pleskin*, where the perpendicular part at the summit is 170 feet, and the precipitous part from the bottom of the pillars to the sea 200. As we proceed on from *Pleskin* to *Dunseverick*, the height gradually abates, and is finally reduced to about 100 feet.

In this whole space, wherever the precipice is accurately perpendicular, the several strata are easily distinguished from each other, but where the slightest obliquity prevails, a grassy covering is formed that effectually conceals all beneath it; hence the face of the precipice seems much diversified; the columnar strata in some places only exhibiting detached groups of pillars, while in others they form extensive colonnades.

I shall now state the appearances as we approach, and coast the promontory from the westward, noticing in this first view of the precipice, every thing that may be considered as general, and reserving (as I did with you) for my return in the contrary direction, a detailed account of the strata taken separately.

The first circumstance, that occurs to the attentive observer

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server on his approach, is, that although both the promontory itself, and the strata composing it, ascend to the northward, yet it is not in the same angle, the strata being more inclined to the horizon than the line tracing the surface of the promontory, a fact which I shall account for afterwards.

From the *Black Rock* to the *Giant's Causeway* (about a mile) the materials, and their arrangement, are similar to those of the coast to the westward, viz. strata of table basalt, generally separated by thinner strata of a reddish substance.

At the *Giant's Causeway* a new arrangement commences, one of the little systems I have mentioned in other memoirs, by the aggregate of which our coast is formed; nature having changed her materials, or their disposition, or both, every two or three miles. To the system of strata comprehended between the *Giant's Causeway* and *Dunseverick* I now limit myself, as all the strata composing it emerge between these two points.

As we proceed along the coast from the *Giant's Causeway* eastward, we perceive the whole mass of strata ascend gradually, culminate at the northern point of the promontory, and then descend more rapidly, as the land falls away to the south-east, until having traced them across the face of the precipice we see them immerge separately at and beyond *Portmoon Whyn Dykes*.

The western side of the promontory is cut down perpendicularly, by eleven *Whyn Dykes;* the intervals between them are unequal, but they all reach from the top of the precipice to the water, out of which some of them again emerge in considerable fragments; they are all constructed of horizontal prisms, which are strongly contrasted with the vertical pillars of the strata through which they pass.

One of the dykes at *Port Cooan,* on *Bengore,* half a mile from the *Giant's Causeway,* is very beautiful; an insulated rock about 160 feet high, and 20 in diameter, stands perpendicular in the middle of a small bay; the main body of the rock is similar to the contiguous consolidated masses; but on the east side a singular whyn dyke is joined to it, composed (as they often are) of several walls agglutinated together, with wall-like fragments of other parts of the dyke

emerging

emerging at their base; the solid mass of dyke is seen cutting down the precipice to the southward at 150 yards distance.

Depression of the Strata.

Soon after we have passed the last of our whyn dykes at *Port Spagna,* (a name derived from a vessel belonging to the Spanish armada having been driven ashore in that creek,) we discover a new and curious circumstance, viz. that the western half of the promontory has sunk or subsided between thirty and forty feet, without the slightest concussion or derangement of the parallelism of the strata.

Two other depressions appear as we proceed onwards, one at *Portmoon,* and the other at the angle where the promontory begins to project from the rectilineal coast; these however are far less considerable in thickness than the preceding, neither of them exceeding five feet.

Such depressions occur at the collieries near *Ballycastle,* and generally on one side of a whyn dyke. We have also at *Seaport,* two miles west from the *Giant's Causeway,* a dyke oblique and undulating, with a depression of the strata of about four feet on one side; but on *Bengore* promontory our dykes are unaccompanied by depressions of the strata, and where we have depressions we do not find a trace of a dyke.

The portions of this extensive façade, which I have selected for explanatory views, are *Portmoon,* in or near which most of the strata emerge, and *Pleskin,* where the strata culminate: each of these views, too, exhibits one of our depressions; but in that of *Pleskin* the first apparent depression is purely an optical effect arising from the position of my friend major O'Neal, of the 56th, who took his view from the water.

Enumeration of the sixteen Strata that compose the Promontory of Bengore, *taken in their regular Order, and counting from above.*

The country immediately to the southward of *Bengore* is like the promontory itself, a stratified mass, accumulated to the summits of *Craig Park* and *Croaghmore,* the first five hundred and the second seven hundred feet high; but with these strata I have nothing to do, limiting myself to those alone of which the promontory is formed, and which are exhibited in its façades.

The uppermost of these commences near half a mile to the eastward of the angle, where the coast deflecting from its due east and west course, turns to the north-west, and begins to form the promontory.

So far the course of this stratum is to appearance perfectly horizontal; for the strata all ascending to the north, the intersection of their planes with the plane of the sea must run east and west, that is, in the present case it coincides with the direction of the coast.

But when the coast changes its direction, this coincidence ceases, and the façade (that is the vertical section of the coast) losing its east and west course, its strata must appear to ascend towards the point it turns to; therefore the strata at *Portmoon*, and along the north-east side of the promontory, should ascend obliquely along the façades, as they actually do.

First Stratum, (m).

The stratum I commence with forms the whole façade, from its first appearance until it reaches the promontory; it consists of massive pillars rather rude, and about sixty feet long, its course for half a mile (as I have stated) seems horizontal, but on the face of the promontory it ascends, and continues to rise uniformly until it reaches the summit, which it lines as far as *Portmoon*, on the south side of which it loses some of its thickness, then suddenly disappears and vanishes from that façade, receding westward in the form of a stony ridge, and is seen no more.

Second Stratum, (k).

The stratum upon which the preceding rests, is red as brick, and about nine feet thick; it appears in spots and patches just above high water mark, so long as the incumbent stratum continues horizontal, but when that rises obliquely, the second ascends with it; it is now completely displayed, and having supported the preceding in its course to the summit, vanishes with it (at *x* in the view of *Portmoon*), and is seen no more.

These ochreous matters, so common in all basaltic countries, according to Mr. F. St. Fond's opinion, were once pure basalt, but have undergone some chemical process of nature we are unacquainted with, by which their colour has been changed.

Third Stratum, (*i*).

The next stratum is the last of those composing the promontory which appears beyond it; for so long as the first and second continue their horizontal course towards *Bengore*, this third accompanies them, showing its upper surface between high and low water-mark; but when it ascends along with the others across the façades it displays its whole thickness, above fifty feet.

This stratum is of that variety of basalt, I have on different occasions distinguished by the name irregular prismatic; it resembles the columnar basalt in grain, but differs from it totally in principle of internal construction, for its prisms are small, not articulated, and indifferent as to the position of their axes, which is perpetually changing.

The irregular prismatic basalt accompanies the columnar in most countries, as at *Pont du Baume*, at *Trezza*, at *Bolsena* in the *Sound of Mull*, and at *Staffa*. In *Antrim*, it is very common; and here is a striking resemblance between the rock crowning the celebrated columns at *Staffa*, and a stratum covering a very neat colonnade at *Craigahullur*, near *Portrush*.

This stratum (as is well exhibited in the view of *Portmoon*) is scolloped off irregularly from the point where it becomes superficial (*x*), until it completely disappears at (*z*); a thin stripe of its lower edge alone is ever resumed again.

Fourth Stratum, (*h*).

The next three strata will require only very short descriptions; the fourth is about seven feet thick, entirely columnar, the pillars small, but not neat: they appear very white from a thick covering of *Byssus saxatilis*, which shows a great predilection for this stratum.

Fifth

Fifth Stratum, (g).

This stratum is ochreous, and more of a slate colour than any of the other red strata; as it is friable, it soon acquires a grassy coat, through which it is only in spots that it shows its proper colour; it is about eight feet thick.

Sixth Stratum, (*f*).

This stratum is composed of rude massive pillars so coarsely formed, that on the least abatement of perpendicularity the columnar form can scarcely be traced. This stratum is about ten feet thick, it forms the vertex of the beautiful conical island *Beanyn Daana*, and is marked in the views (*f*).

These last strata, though they have nothing very remarkable in themselves, nor contribute much to the beauty of the façade; yet they exhibit one of the most important facts I am acquainted with in natural history, and which, when attentively considered, throws much light on the nature of the operations performed upon our globe since its consolidation, and leads us irresistibly to conclusions extraordinary and unexpected.

The fourth, fifth, and sixth strata reach the top of the precipice, and vanish together at the waterfall in the north-west corner of *Portmoon*. When they come to the surface, they turn inland to the westward in long stony ridges; these obstruct the course of the waters in their descent along the inclined plane, formed by the surface of the promontory, and throw them over the precipice, in a cascade highly beautiful after rain.

On the façades to the north-west not a trace of them appears, these being entirely formed by the lower strata, which I have not yet noticed; but at the distance of a mile, at the great depression (already mentioned), the fourth, fifth, and sixth strata, with a narrow stripe of the third, suddenly appear, in their regular posts, their proper order, and with all the characteristic marks peculiar to each separate stratum.

In the interval between the depression at *Pleskin*, and the *Giant's Causeway* (about a mile), these three strata often appear in a desultory way on the summit of the precipice, wherever it is of sufficient height to receive them, always preserving their usual thickness, their characters, and their order; so that a person master of the order I am detailing, as he approaches a rising point of the precipice, can tell its strata, and their order, before he is near enough to distinguish them.

Seventh Stratum, (d).

The rude and massive pillars of the sixth stratum pass into the neater, and much longer columns of the seventh, without interrupting the solidity or continuity of the material; exactly as a down-held hand appears to separate into fingers. The thickness of this stratum, that is the length of the pillars of which it is formed, is fifty-four feet; it is marked (*d*) in the two views, and in its passage across the face of the precipice, displays more beautiful colonnades than any of the others.

This seventh stratum emerges from the beach immediately behind the south-east point of *Portmoon*, and where it first shows itself in that bay, has its lower edge raised only a few feet above the water; it forms the upper frustum of the larger of the two conical islands, ascends obliquely along the face of *Portmoon*, and continues to rise until it composes the upper range in the beautiful façade, properly called *Bengore Head*. This is probably the most magnificent of all, its convexity towards the sea producing a fine effect. The lower edge of this stratum, that is the line forming the base of its pillars, has here, as at *Pleskin*, attained the height of three hundred feet above the water.

The seventh stratum, like those above it, also suffers an interruption; for after having exhibited itself to such great advantage at *Bengore*, the extreme northern point of the promontory lowers, and this stratum disappears for about one-third of a mile; as the promontory rises, it is resumed again in great beauty at *Pleskin*, and is interrupted no more; we scarcely ever lose sight of it until we reach *Port Noffer* (the next bay to the Causeway); here, for want of perpendicularity it is little seen, and is finally lost over the causeway, we know not well how.

Eighth

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Eighth Stratum, (c).

The next stratum is of the same variety of basalt with the third, that is, irregular prismatic; it is fifty-four feet thick, and in the views distinguished by the letter (c): where it emerges at the south-east corner of *Portmoon*, it is quite accessible by land, and affords the best opportunity I know for examining this species of basalt, as it is there very neat.

There is little more of this stratum seen in the façade of *Portmoon* for want of perpendicularity, but it forms the lower frustum of the great conical island *Beanyn Daana*, and the whole of the smaller, except the base; it is well displayed over the remainder of the precipice, it forms the intermediate stratum between the magnificent colonnades at both *Bengore* and *Pleskin*, and finally is lost just over the *Giant's Causeway*. Large globular fragments have fallen from it, and are scattered about the causeway.

Ninth Stratum, (b).

This stratum is forty-four feet thick, that being the exact length of the neat pillars composing it; at its emersion it forms the bases of the two conical islands in *Portmoon*, and is no more seen in that bay, but immediately to the northward it begins to show itself in colonnades and groups, some of them resembling castles and towers.

It ascends along the precipice obliquely, like those above it, forms the lower range at *Bengore* and *Pleskin*, from which last it dips to the westward regularly, composes the group at *Port Noffer*, called the *Organs*, seen from the causeway, and finally at its immersion, or intersection with the plane of the sea, it forms the beautiful assemblage of neat pillars, so long distinguished by the name of the *Giant's Causeway*.

At these two intersections, each of them accessible by land and water, the prisms exactly resemble each other in grain, size, and neatness; the interval between them is full two miles, through great part of which this stratum is displayed at different heights; it culminates between *Pleskin* and *Bengore*, with its lower edge more than two hundred feet above the water.

We see now what a diminutive portion of our vast basaltic mass has, until lately, monopolized the attention of the curious; and even after it was discovered that we had many other, and much finer collections of pillars on the same promontory, it never occurred to those who were preparing to give accounts of them to the public, to examine whether these were mere desultory groups, or detached parts of a grand and regular whole, which a more comprehensive view of the subject would soon have laid open to them.

Tenth Stratum, (a).

The stratum upon which the pillars of the preceding rest, is ochreous, red as minium, and about twenty feet thick; it is scarcely seen at *Portmoon*, a patch alone of its surface being distinguishable under water at low tide; but immediately to the northward it shows itself, and from its bright colour, makes a conspicuous figure across the face of the precipice in a course of more than a mile and half; its last appearance to the westward is at *Rovinvalley*, the opposite point of the bay from the *Giant's Causeway*, from which we have a good view of it. The final dip and immersion of this tenth stratum, as well as its emersion, are lost for want of perpendicularity.

The six remaining strata are all similar in material, but differing much from each other in thickness; they are all of that description called tabular basalt, sometimes showing a faint disposition to assume a columnar form at their edges, and always separated from each other by ochreous layers.

These six strata are not so perfectly distinct as those above them, for sometimes we think we can count seven, and again not more than five; nor does each of these preserve the same thickness through their whole extent, for they are deeper towards the northern point, where they culminate, forming by themselves a perpendicular façade near two hundred feet high, but they grow thinner as they recede from this centre.

The jets of black rock in the view of *Portmoon*, are the emersions of these strata; their last appearance on the west side is at *Rovinvalley*, where they strongly display the inclination

nation of their strata, (the same with all the rest) to those approaching from the westward; their final immersion is lost for want of perpendicularity.

I shall now proceed to select from the great mass of facts that are exhibited on the face of *Bengore* promontory, and occur in the contiguous basaltic country, such as seem applicable to geological questions, and likely to throw light on such subjects.

Facts applicable to geological Questions.

1. Every stratum preserves accurately, or very nearly, the same thickness through its whole extent, with very few exceptions.

2. The upper and lower surface of each stratum preserve an exact parallelism, so long as they are covered by another stratum; but when any stratum becomes the superficial one, its upper surface is scolloped, or sloped away irregularly, while the plane forming its base continues steady and rectilineal; but the parallelism of its planes is resumed as soon as another stratum is placed over it.

3. The superficial lines bounding the summit of our façades, and our surface itself, are unconnected with, and unaffected by, the arrangement of the strata below them.

4. Nature, in the formation of her arrangements, has never acted upon an extensive scale in our basaltic area, (at least on its northern side, where our continuous precipices enable us to determine the point with precision,) but changes her materials, or her arrangement, or both, every two or three miles, and often at much smaller intervals.

5. Wherever there is a change of material, as from one stratum to another in a vertical line; or where the change is in a horizontal direction by the introduction of a new system; or where a whyn dyke cuts through an accumulation of strata; in all these cases the change is always *per saltum* and never *per gradus*, the lines of demarcation always distinct, and well defined; yet the different materials pass into each other without interrupting the solidity and continuity of the whole mass.

6. The façades on our coast are formed as it were by ver-

tical planes, cutting down, occasionally, the accumulations of our strata; the upper part of these façades is generally perpendicular, the lower steep and precipitous.

7. The bases of our precipices commonly extend a considerable way into the sea; between the water and the foot of the precipice, (and especially near the latter,) there is frequently exhibited the wildest and most irregular scene of confusion, by careless observers supposed to be formed by the ruins of the precipice above, which have fallen down: such, no doubt, was Mr. Whitehurst's idea, when he describes one of these scenes as "an awful wreck of the terraqueous globe."

But a more attentive observer will soon discover that these capricious irregularities, whether in the form of rude cones, as at *Beanyn Daana*, and the west side of *Pleskin*; or towers, as at the dyke of *Port Cooan* and *Castro Levit*, at the foot of *Magilligan* façade, even spires and obelisks, as to the westward of *Kenbaan*, and at the *Bull of Rathlin*; yet all of these once formed part of the original mass of coast, stratified like it, and their strata still correspond in material and inclination with those in the contiguous precipice.

8. These vertical sections or abruptions of our strata are by no means confined to the steeps that line our coast; the remaining boundary of our basaltic area has several of them equally grand; and similar abruptions, or sections, (though not so deep,) are scattered over a great part of our area, and especially on the ridges of our hills and mountains which are cut down in many places like a stair, by the sudden abruption of the basaltic stratum.

9. Wherever the strata are thus suddenly cut off, whether it be a mass of accumulated strata as in the façades on our coast, or solitary strata in the interior; the materials on one side of the abruption are completely carried away, without a fragment being left behind, while on its other side the untouched stratum remains entire and undisturbed.

I shall not proceed to apply these facts to support, or invalidate, any of the numerous theories which have given rise to so much controversy, in which I myself (as you know)

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know) have borne some part; I shall look to nature alone, without much reference to opinions, and shall endeavour to trace, by the marks she has left behind her, some of the grand operations she once executed on the surface of our globe.

Varro divided the time elapsed since the beginning of the world into three portions, which he distinguished by the names; *prolepticum*, *fabulosum*, and *historicum*.

The first comprehended the period of absolute darkness; in the second some faint lights were thrown upon the history of its events, by fable and tradition; in the third, the historian had the common aids from which history is usually compiled.

The natural history of the world seems to admit of a corresponding division. In the first I include the formation of our strata, their induration, their derangement from the horizontal position in which they seem originally to have been placed, and the operation of cutting them down by so many whyn dykes.

In the second division, corresponding to Varro's *fabulosum*, I comprehend the operations performed upon our globe, posterior to its final consolidation, and antecedent to all history or tradition; operations therefore that can be established by the visible effects alone which still exist, written in strong characters.

The third division contains the period since we acquired some knowledge of natural history, became acquainted with causes and effects, and able to trace the connection between them.

With the operations performed in the first division (corresponding with Varro's *prolepticum*) modern theorists assume that they are well acquainted, able to account for every appearance, and to detail the whole process of original formation. I however shall decline noticing these early processes of nature, and limit myself to the second division of natural history, hoping from the prominent features of my country that remain still undefaced, and from its curious facts, to trace and demonstrate the great effects that have been produced upon our surface; and though I do not presume

sume to advance further, I perhaps may assist in clearing the way for future naturalists, and, by establishing effects, encourage them to proceed to causes, and help them to discover the powers and agents by which these grand operations have been executed.

[To be continued.]

XIX. *Result of some Experiments on the Distillation of various Vegetable and Animal Substances in the dry Way.*

By DAVID MUSHET, *Esq.*

[Continued from p. 10.]

Experiment XXIII.

Bitter Almonds, 240 grains.—In distillation they discharged a great quantity of smoky flame. The almonds were found without adhesion to each other, and contained upon their surface a great variety of prismatic colours. The coal was found to weigh 32 grains: 208 grains having been lost by the distillation.

Component parts:	Volatile matter	80·66
	Oxide of carbon	19·34
		100 parts.

Experiment XXIV.

Black Pepper, not ground, 240 grains.—In distilling, a considerable portion of flame was disengaged. The coal obtained was partially prismatic. Every spherule of pepper preserved its original form. A few masses slightly adhered together without any appearance or reality of welding. Weight of the coal 53 grains. Loss by distillation 187 grs.

Component parts:	Volatile matter	77·91
	Oxide of carbon	22·09
		100 parts.

Experiment XXV.

White Pepper, 240 grains.—This substance flamed a good deal in distilling, of a pure white colour, edged with pale blue. The product in coal was clear, and shining, each corn entire, and partially connected in groups, which, however, separated upon being exposed to air. Weight 50 grains; having lost by distillation 190 grains.

Component parts:	Volatile matter	79·16
	Oxide of carbon	20·84
		100 parts.

Experiment XXVI.

Fine Black Tea, 240 grains.—The result of this distillation was a slightly prismatic coal, to appearance very little altered in shape, bulk, or colour. It weighed 74 grains. Loss in distillation 166 grains.

Component parts:	Volatile matter	69·16
	Oxide of carbon	30·84
		100 parts.

Experiment XXVII.

Gunpowder Tea, 120 grains.—This substance afforded a small portion of flame during the operation of distilling. The result was a firm compact coal, thoroughly welded together by means of a silvery-gray species of coal, perhaps of resinous extraction. The whole surface of the mass, which resembled in shape the interior of the retort, was covered with fine prismatic shades. The grains of tea had not, though welded, lost any part of their shape. The coal thus obtained weighed 39 grains. Loss by distillation 81 grains.

Component parts:	Volatile matter	67·5
	Oxide of carbon	32·5
		100 parts.

Experiment XXVIII.

Coffee Beans, 240 grains.—The distillation of this substance disengaged a considerable portion of flame. The beans were found in the state of a beautiful prismatic coal of the most lively and elegant shades. The whole were destitute of adhesion, and seemed to possess nothing of the welding property.

perty. They now weighed 45 grains, having lost 195 grains by distillation.

Component parts:	Volatile matter	81·25
	Oxide of carbon	18·75
		100 parts.

Experiment XXIX.

Dutch Cheese, 270 grains.—After a distillation, which was attended with the discharge of a watery-coloured smoky flame, a rough sooty coal was found, without any symptoms of welding. It was salt to the taste, and in distilling emitted strong fumes of muriatic acid gas. The coal weighed 48 grains, 222 having been lost by distillation.

Component parts:	Volatile matter	82·22
	Oxide of carbon	17·78
		100 parts.

Experiment XXX.

Scotch Cheese (Dunlop), 300 grains.—This cheese flamed violently during the distillation. The colour of the flame approached near to that of thick oil, and deposited a considerable portion of soot. The resulting coal was much shrivelled, possessed nothing of the welding property, and weighed 25 grains, having lost by distillation 275 grains. The colour of this coal was grayish black, and saltish to the taste. It was not, however, nearly so pungent as that obtained from the Dutch cheese in the former experiment.

Component parts:	Volatile matter	91·66
	Oxide of carbon	8·34
		100 parts.

Experiment XXXI.

Cheshire Cheese, 300 grains.—The discharge of flame in the distillation of this cheese was still more violent than in the two former. The coal was reduced to a mere skeleton. The colour brownish black, with a small perception of salt. It weighed 15 grains. Loss 285 grains.

Component parts:	Volatile matter	95
	Oxide of carbon	15
		100 parts.

It would appear by the Experiments from Exper. XIII. inclusive, with the exception of the gunpowder tea, that the coals obtained from these various substances do not contain the welding or caking principle; and so far they resemble the residual coals, or oxides obtained from the distillation of all the woods.

The following experiments are selected from a variety made with animal substances.

Experiment XXXII.

Beef, entirely freed from fat, 1121 grains.—This in distillation gave out a light blue penetrating smoke, which towards the middle of the operation became particularly offensive. Towards the close a blue lambent flame appeared, which continued to twitter for some time; after this, all offensive smell ceased, and a skeleton coal of a pure black colour was obtained perfectly insipid, which weighed 34 grains; 1087 grains having been lost by distillation.

Component parts:	Volatile matter	97
	Oxide of carbon	3
		100 parts.

Experiment XXXIII.

Fat, cut from the same beef, 425 grains.—This was slowly distilled, and with a moderate heat, so as to restrain the flame. After ignition there was found in the bottom of the retort a long flake of carbonaceous matter that weighed three grains. Loss by distillation 422 grains.

Component parts:	Volatile matter	99·29
	Oxide of carbon	00·71
		100 parts.

Experiment XXXIV.

Mutton, cut from the thick part of a hind-quarter and carefully freed from fat, 775 grains.—This substance exhaled the same disagreeable odour as the beef, but was succeeded by a flame that burnt longer and with more violence. The coal obtained was light and honeycombed, of a dark gray colour, and weighed 23 grains. Loss by distillation 752 grains.

Component parts:	Volatile matter	95·74
	Oxide of carbon	4·28
		100 parts.

Experiment XXXV.

Fat, cut from the same quarter, 225 grains.—A thin flat cake of silvery gray coal was obtained, which weighed 2·25 grains.—Lost in distillation 223·75 grains.

Component parts:	Volatile matter	99
	Oxide of carbon	1
		100 parts.

Experiment XXXVI.

Veal, carefully separated from fat, 1245 grains.—The result of the distillation of veal gave a fine silvery gray coloured coal, light, and extremely spongy, weighing 44 grains.—Loss by distillation 1201 grains.

Component parts:	Volatile matter	96·47
	Oxide of carbon	3·53
		100 parts.

Experiment XXXVII.

Lean of Pork, 820 parts.—This flamed a great deal, and yielded a coal of considerable bulk, but very light and spongy. The colour was silver gray. It weighed 49 parts, having lost by distillation 771.

Component parts:	Volatile matter	94·03
	Oxide of carbon	5·97
		100 parts.

Experiment

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Experiment XXXVIII.

A Toad, weighing 554 grains.—This was introduced into a retort, and distilled with nearly the same attendant circumstances as in the experiments immediately before. At the commencement of the operation a violent hissing was perceptible, accompanied with slight explosions. The result was, the skeleton nearly in the position in which it was originally placed. The flesh and skin had disappeared, and left a very perfect coal adhering to the bones, all of which were converted into a beautiful white colour.—The carbonaceous matter weighed 24 grains; the bones, some of which soon fell into powder, 7 grains:—total 31 grains. Loss in the distillation 523 grains.

Toad:	Volatile matter	94·40
	Lime -	1·26
	Oxide of carbon	4·34
		100 parts.

Experiment XXXIX.

An Eel, weighing 704 grains, and newly killed, was boiled up and put into the retort, and exposed till all smell of animal had ceased. A mass of coal was found attached to the slender bones of the animal, and in the same position it originally occupied. It was not easily possible to separate the bone. The whole weighed 28 grains, having lost in distillation 676 grains.

Component parts of eel:	Volatile matter	96
	Oxide of carbon	4
		100 parts.

Experiment XL.

White Snails, 340 grains.—These in distillation formed a pretty firm mass of caked coal of a dull black colour. The same penetrating smell of animal matter was perceived in this experiment as in the former. The coal weighed 21 grains; 319 grains having been lost by distillation.

Component parts of white snails:	Volatile matter	93·82
	Oxide of carbon	6·18
		100 parts,

Experiment XLI.

Black Snails, 637 grains.—A shrivelled coal was found to be the result of the distillation of these animals, mixed with nearly one-third part of caustic lime of a gray earthy colour: the whole weighed 50 grains; the loss by distillation having been 587.

Component parts of black snails:	Volatile matter	92·15
	Oxide of carbon	7·85
		100 parts.

Experiment XLII.

Earth Worms, that had been kept for some weeks to cleanse themselves, 1000 grains.—A considerable quantity of gas was evolved during the distillation, and the usual smell of the combustion of animal matter. A firm bulky mass of coal weighing 130 grains was obtained, mixed with concretions of earthy matter. Loss by distillation 870 grains.

Component parts:	Volatile matter	87
	Oxide of carbon mixed with earths	13
		100 parts.

It is rendered highly probable from these experiments, that every animal substance contains a portion of oxide of carbon; and many of those that exhibit but a small residuum in the state of coal, part with a considerable quantity in the state of gas and flame, dissolved in hydrogen, but which is lost from a recompounding affinity not being present at the period of disengagement. From this circumstance most probably may arise, in part, a well established fact, that the same quantity of any of the substances before operated upon, will communicate a greater share of carbonaceous matter when in a raw state, than the same substance carefully converted into charcoal, and afterwards applied to the same purpose. The substances being so varied and numerous that contain the oxide of carbon, it could not possibly follow that the extent of their carbonaceous effects would be in the proportion of their masses under similar circumstances, nor that the charcoal or oxide that each of them afforded by distillation would bear the same relation to each other in point of purity, even where the acknowledged quantity of alloy in the state of earths, salts, &c., were the same. We therefore find, that even in the state of the raw substance equal weights or quantities of matter, calculating always upon the residuum alloys of different substances, produce results materially different, which can only be attributable to the different existing state of the oxide, or to the decomposition of the hydro-carbonate which most of them contain. The difference of the results becomes much greater when the oxide of carbon is used in the state of charcoal or coke, and the variety of the results here also obtained, where no hydro-carbonate comes into action, can only be placed to the state of oxidation of the carbon.

In a future communication I shall illustrate, by some experiments, what I have just stated.

XX. *Hydraulic Investigations, subservient to an intended Croonian Lecture on the Motion of the Blood.* By THOS. YOUNG, *M.D. For. Sec. R.S.**

I. *Of the Friction and Discharge of Fluids running in Pipes, and of the Velocity of Rivers.*

HAVING lately fixed on the discussion of the nature of inflammation, for the subject of an academical exercise, I found it necessary to examine attentively the mechanical principles of the circulation of the blood, and to investigate minutely and comprehensively the motion of fluids in pipes, as affected by friction, the resistance occasioned by flexure, the laws of the propagation of an impulse through the fluid contained in an elastic tube, the magnitude of a pulsation in different parts of a conical vessel, and the effect of a contraction advancing progressively through the length of a given canal. The physiological application of the results of these inquiries I shall have the honour of laying before the Royal Society at a future time; but I have thought it advisable to communicate, in a separate paper, such conclusions, as may be interesting to some persons, who do not concern themselves with disquisitions of a physiological nature; and I imagine it may be as agreeable to the Society that they should be submitted at present to their consideration, as that they should be withheld until the time appointed for the delivery of the Croonian Lecture.

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* From Philosophical Transactions for 1808.

It has been observed by the late Professor Robison, that the comparison of the Chevalier Dubuat's calculations with his experiments is in all respects extremely satisfactory; that it exhibits a beautiful specimen of the means of expressing the general result of an extensive series of observations in an analytical formula, and that it does honour to the penetration, skill, and address of Mr. Dubuat, and of Mr. de St. Honoré, who assisted him in the construction of his expressions. I am by no means disposed to dissent from this encomium; and I agree with Professor Robison, and with all other late authors on hydraulics, in applauding the unusually accurate coincidence between these theorems and the experiments from which they were deduced. But I have already taken the liberty of remarking, in my lecture on the history of hydraulics, that the form of these expressions is by no means so convenient for practice as it might have been rendered; and they are also liable to still greater objections in particular cases, since, when the pipe is either extremely narrow, or extremely long, they become completely erroneous: for notwithstanding Mr. Dubuat seems to be of opinion, that a canal may have a finite inclination, and yet the water contained in it may remain perfectly at rest, and that no force can be sufficient to make water flow in any finite quantity through a tube less than one twenty-fifth of an inch in diameter; it can scarcely require an argument to show, that he is mistaken in both these respects.

It was therefore necessary for my purpose to substitute, for the formulæ of Mr. Dubuat, others of a totally different nature; and I could follow Dubuat in nothing but in his general mode of considering a part of the pressure, or of the height of a given reservoir, as employed in overcoming the friction of the pipe through which the water flows out of it; a principle, which, if not of his original invention, was certainly first reduced by him into a practical form. By comparing the experiments, which he has collected, with some of Gerstner's, and some of my own, I have ultimately discovered a formula, which appears to agree fully as well as Dubuat's with the experiments from which his rules were deduced, which accords better with Gerstner's experiments, which extends to all the extreme cases with equal accuracy, which seems to represent more simply the actual operation of the forces concerned, and which is direct in its application to practice, without the necessity of any successive approximations.

I began by examining the velocities of the water discharged through pipes of a given diameter with different degrees of pressure; and I found, that the friction could not be represented by any single power of the velocity, although it frequently approached to the proportion of that power of which the exponent is 1·8; but that it appeared to consist of two parts, the one varying simply as the velocity, the other as its square. The proportion of these parts to each other must however be considered as different in pipes of different diameters, the first part being less perceptible in very large pipes, or in rivers, but becoming greater than the second in very minute tubes; while the second also becomes greater for each given portion of the internal surface of the pipe, as the diameter is diminished.

If we express, in the first place, all the measures in French inches, calling the height employed in overcoming the friction f, the velocity in a second v, the diameter of the pipe d, and its length l, we may make $f = a\frac{l}{d}v^2 + 2c\frac{l}{d}v$; for it is obvious, that the friction must be directly as the length of the pipe; and since the pressure is proportional

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to the area of the section, and the surface producing the friction to its circumference or diameter, the relative magnitude of the friction must also be inversely as the diameter, or nearly so, as Dubuat has justly observed. We shall then find that a must be ${\cdot}0000001 \left(430 + \frac{75}{d} - \frac{1440}{d+12} - \frac{180}{d+\frac{1}{3}}\right)$; and $c = {\cdot}0000001 \left(\frac{900dd}{dd+1000} + \frac{1}{\surd d}\left(1050 + \frac{12}{d} + \frac{{\cdot}9}{dd}\right)\right)$. Hence it is easy to calculate the velocity for any given pipe or river, and with any given head of water. For the height required for producing the velocity, independently of friction, is, according to Dubuat, $\frac{v^2}{478}$, or rather, as it appears from almost all the experiments which I have compared, $\frac{v^2}{550}$: and the whole height h is therefore equal to $f + \frac{v^2}{550}$, or $h = \left(\frac{al}{d} + \frac{1}{550}\right) v^2 + \frac{2cl}{d} v$; and making $b = \frac{1}{al:d + 00182}$, and $e = \frac{bcl}{d}$, $v^2 + 2ev = bh$, whence $v = \surd(bh + e^2) - e$. In order to adapt this formula to the case of rivers, we must make l infinite; then b becomes $\frac{d}{al}$ and $bh = \frac{d}{a} \cdot \frac{h}{l} = \frac{ds}{a}$, s being the sine of the inclination, and d four times the hydraulic mean depth; and since e is here $= \frac{c}{a}$, $v = \frac{\surd(ads + cc) - c}{a}$, and in most rivers, v becomes nearly $\surd(20000\,ds)$.

In order to show the agreement of these formulæ with the result of observation, I have extracted, as indiscriminately and impartially as possible, forty of the experiments made and collected by Dubuat; I have added to these some of Gerstner's, with a few of my own; and I have compared the results of these experiments with Dubuat's calculations, and with my own formulæ, in separate columns. There are six of Dubuat's experiments, which he has rejected as irregular, apparently without any sufficient reason, since he has accidentally mentioned, that some of them were made with great care: I have therefore calculated the velocities for these experiments in both ways, and compared the results in a separate table.

Tabular

Tabular Comparison of Hydraulic Experiments.

Observer.	*d.*	$\frac{1}{s}$	Superf. Veloc.	*v.*	Dub.	Log. ratio.	Y.	Log. ratio.	*a* ·17 ×	*c* ·17 ×	√ (20000*ds*)
DUBUAT.	262·5	35723	15·96	12·56 ?	10·58	·0776	11·10	·0537	424	952	11·1
	258·5	6413	31·77	26·63 ?	28·76	·0334	28·02	·0221	424	952	28·3
	92·4	21827	9·61	7·01 ?	8·38	·0775	8·14	·0619	415	911	9·3
	75·6	27648	7·27	5·07 ?	6·55	·1113	6·27	·0923	413	887	7·5
	17·6	9288		5·70	5·86	·0120	5·97	·0291	376	465	6·1
	16·	432		32·52	31·61	·0124	30·67	·0255	374	451	27·6
	11·7	1412		14·17	13·59	·0182	14·05	·0037	360	416	12·2
	9·9	427		22·37	24·37	·0372	24·41	·0379	355	414	21·7
	5·8	91[illegible]		27·51	27·10	·0051	27·84	·0027	332	266	23·5

Observers.	*d.*	*l.*	*h.*	*v.*	Dub.	Log. ratio.	Y.	Log. ratio.	*a* ·17 ×	*c* ·17 ×
COUPLET.	18	43200	145·08	39·16	40·51	·0148	38·49	·0075	376	469
	5	84240	25·00	5·32	5·29	·0024	[illegible]	·0065	326	492
			16·75	4·13	4·23	·0103	4·21	·0083		
			5·58	2·01	2·25	·0490	2·04	·0066		
BOSSUT.	2·01	2160	24	24·73	24·08	·0115	24·76	·0006	287	747
			12	16·38	16·10	·0075	16·86	·0125		
		1080	24	35·77	35·10	·0082	35·05	·0089		
		360	24	58·90	58·80	·0007	56·8	·0154		
	1·33	2160	12	12·56	12·75	·0065	13·28	·0242	270	919
		1080	24	28·08	28·21	·0020	28·84	·0116		
		360	24	48·53	49·52	·0088	48·66	·0015		
	1·	600	12	22·28	21·98	·0055	22·85	·0106	259	1063
			4	12·22	11·76	·0167	11·92	·0108		
DUBUAT.		737	23·7	28·67	29·11	·0111	30·11	·0213		
			12·2	19·99	19·95	·0009	20·67	·0145		
			4·2	10·56	10·66	·0041	10·90	·0137		
		117	26	84·95	85·52	·0029	83·12	·0069		
			18	58·31	58·47	·0014	58·41	·0012		
	·24167	36·25	53·25	85·77	85·20	·0029	85·71	·0003	309	2268
			41·25	73·81	73·90	·0005	74·67	·0050		
			20·47	51·96	50·14	·0155	50·87	·0093		
			5·00	23·20	23·19	·0039	23·09	·0058		
			·83	7·58	8·22	·0420	7·29	·0212		
	·1667	36·25	51·25	64·37	61·95	·0081	64·08	·0021	402	2827
			38·75	51·19	55·32	·0690	51·93	·0055		
			15·29	33·36	33·17	·0028	32·67	·0094		
			2·01	10·62	10·49	·0053	9·2	·060		
	·125	34·17	42·17	45·47	46·21	·0070	45·89	·0039	518	3405
			35·33	41·61	41·71	·0010	41·56	·0006		
			14·58	26·20	25·52	·0114	24·9	·0214		
			2·05	7·32	8·35	·0572	6·9	·0206		

(Mean ·0176 (Mean ·0169
= L. 1·042) = L. 1·040)

Observers.	*d.*	*l.*	*h.*	*v.*	Dub.	Log. rat.	Y.	Log. rat.	*a.*	*c.*
Gerstner at 55·5° F.	·2	63	10·7	24·2	23·9	006	24·1	·002	349	2533
			7·7	21·0	19·9	·023	19·1	·042		
			4·7	15·8	14·9	·026	13·9	·056		
			1·7	7·5	8·2	·039	6·9	·036		
			·7	2·5	5·0	·301	3·4	·133		
	·133	33	10·7	27·1	23·4	·064	22·5	·081	488	3259
			7·7	23·2	19·4	·077	18·5	·098		
			4·7	15·4	14·9	·024	13·5	·058		
			1·7	5·6	8·1	·160	6·7	·078		
			·7	2·3	4·6	·301	3·4	·169		
	·0674	33	10·7	10·0	8·9	051	10·1	·004	975	5700
			7·7	7·2	7·4	·012	8·2	·057		
			4·7	4·5	5·6	·095	5·6	·095		
			1·7	1·5	3·1	316	2·5	·222		
			·7	·5	1·2	·444	1·1	·342		

(Mean ·129 = L. 1·346 ·098 = L. 1·254)

Observers.	*d.*	*l.*	*h.*	*v.*	Dub.	Log. rat.	Y.	Log. rat.	*a.*	*c.*
Y. at 60°.	$\frac{1}{42}$	8·50	32·4	14·40	0	∞	13·36	·032	2956	13889
	$\frac{1}{123}$	3·42	30·0	·53			·52	·008	13404	452100
		1·17	5·8	·27			·30	·046		

(Mean ·029 = L. 1·068

Observers.	*d.*	*l.*	*h.*	*v.*	Dub.	Log. rat.	Y.	Log. rat.	*a.*	*c.*
Dubuat.	2	255·25	36·35	86·31	84·2	·011	79·7	·035	287	747
	1	24	36·25	122·59	117·8	·018	120·8	·007	259	1063
			27	106·45	101·1	·022	104·1	·010		
			18	84·85	82·2	·013	84·8	000		
			9	59·25	57·5	·013	59·7	·004		
		4	27·08	118·67	111·5	·027	118·5	·000		

(Mean ·017 = L. 1·041 ·009 = L. 1·022

It appears from this comparison, that in the forty experiments extracted from the collection, which served as a basis for Dubuat's calculations, the mean error of his formula is $\frac{1}{24}$ of the whole velocity, and that of mine $\frac{1}{25}$ only; but if we omit the four experiments, in which the superficial velocity only of a river was observed, and in which I have calculated the mean velocity by Dubuat's rules, the mean error of the remaining 36 is $\frac{1}{35}$, according to my mode of calculation, and $\frac{1}{37}$ according to Mr. Dubuat's; so that, on the whole, the accuracy of the two formulæ may be considered as precisely equal with respect to these experiments. In the six experiments which Dubuat has wholly rejected, the mean error of his formula is about $\frac{1}{24}$, and that of mine $\frac{1}{45}$. In fifteen of Gerstner's experiments, the mean error of Dubuat's rule is one third, that of mine one fourth; and in the three experiments which I made with very fine tubes, the error of my own rules is one fifteenth of the whole, while in such cases Dubuat's formulæ completely fail. I have determined the mean error by adding together the logarithmic ratios of all the results, and dividing the sum by the number of experiments. It would be useless to seek for a much greater degree of accuracy, unless it were probable, that the errors of the experiments themselves were less than those of the calculations; but if a sufficient number of extremely accurate and frequently repeated experiments could be obtained, it would be very possible to adapt my formula still more correctly to their results.

In order to facilitate the computation, I have made a table of the coefficients *a* and *c* for the different values of *d*, all the measures being still expressed in French inches.

Table of Coefficients for French Inches.

d	a $\cdot 1^7 \times$	c $\cdot 1^7 \times$	d	a $\cdot 1^7 \times$	c $\cdot 1^7 \times$	d	a $\cdot 1^7 \times$	c $\cdot 1^7 \times$
∞	430	900	15	370	427	·7	249	1278
500	427	943	10	354	414	·6	248	1384
400	426	946	9	350	421	·5	249	1524
300	423	950	8	345	433	·4	257	1717
200	421	951	7	340	440	$\frac{1}{3}$	268	1895
100	416	923	6	335	462	·3	279	2008
90	415	911	5	325	512	$\frac{1}{4}$	303	2225
80	413	896	4	319	540	·2	349	2532
70	410	872	3	305	617	$\frac{1}{6}$	402	2827
60	408	840	2·5	296	687	·15	440	3026
50	406	792	2	288	751	$\frac{1}{7}$	458	3116
40	400	719	1·5	275	866	$\frac{1}{8}$	518	3405
30	393	618	1	259	1063	$\frac{1}{9}$	589	3693
25	387	560	·9	255	1123	·1	646	3985
20	380	492	·8	252	1193			

For example, in the last experiment, where d is 1, l 4, and h 27·1, we have $a = \cdot0000259$, $b = \frac{1}{al : d + \cdot00182} =$

516, $c = {\cdot}0001063$, $e = bcl : d = {\cdot}22$, and $v = \surd(bh + e^2) - e = 118{\cdot}46$, which agrees with the experiment within $\frac{1}{500}$ of the whole. I had at first employed for a the formula $\frac{430}{1 + 12 : d} + \frac{57}{d} + \frac{1}{6dd}$, but I found that the value, thus determined, became too great when d was about 20, and too small in some other cases. Coulomb's experiments on the friction of fluids, made by means of the torsion of wires, give about ·00014 for the value of c, which agrees as nearly with this table, as any constant number could be expected to do. I have however reason to think, from some experiments communicated to me by Mr. Robertson Buchanan, that the value of a, for pipes about half an inch in diameter, is somewhat too small; my mode of calculation, as well as Dubuat's, giving too great a velocity in such cases.

If any person should be desirous of making use of Dubuat's formula, it would still be a great convenience to begin by determining v according to this method; then, taking $b = \frac{l}{h - v^2 : 478}$, or rather, as Langsdorf makes it, $b = \frac{l}{h - v^2 : 482}$, to proceed in calculating v by the formula $v = 148{\cdot}5\,(\surd d - {\cdot}2) \cdot \left(\frac{1}{\surd b - \text{H. L.} \surd (b + 1{\cdot}6)} - {\cdot}001\right)$, since this determination of b will, in general, be far more accurate than the simple expression $b = \frac{l + 45d}{h}$, and the continued repetition of the calculation, with approximate values of v, may thus be avoided. Sometimes, indeed, the values of v found by this repetition will constitute a diverging instead of a converging series, and in such cases we can only employ a conjectural value of v, intermediate between the two preceding ones.

Having sufficiently examined the accuracy of my formula, I shall now reduce it into English inches, and shall add a second table of the coefficients, for assisting the calculation. In this case, a becomes $\cdot 0000001 \left(413 + \frac{75}{d} - \frac{1440}{d + 12{\cdot}8} - \frac{180}{d + {\cdot}355}\right)$, $c = {\cdot}0000001 \left(\frac{900dd}{dd + 1136} + \frac{1}{\surd d}\right.$

(1085

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$\left(1085 + \frac{18{\cdot}21}{d} + \frac{1{\cdot}0563}{dd}\right)$, and $b = \frac{1}{al : d + {\cdot}00171}$, e being $\frac{bcl}{d}$, and $v = \surd\,(bh + e^2) - e$, or $= \surd\left(\frac{ds}{a} + \frac{cc}{aa}\right) - \frac{c}{a}$, as before; and in either case the superficial velocity of a river may be found, very nearly, by adding to the mean velocity v its square root, and the velocity at the bottom by subtracting it.

Table of Coefficients, for English Inches.

d	a $\cdot 1^7 \times$	c $\cdot 1^7 \times$	d	a $\cdot 1^7 \times$	c $\cdot 1^7 \times$	d	a $\cdot 1^7 \times$	c $\cdot 1^7 \times$
∞	413	900	15	354	430	·7	243	1322
500	410	944	10	339	413	·6	243	1433
400	409	948	9	336	421	·5	245	1578
300	406	951	8	331	433	·4	254	1779
200	404	951	7	327	449	$\frac{1}{3}$	268	1963
100	399	918	6	322	471	·3	280	2082
90	398	903	5	312	507	$\frac{1}{4}$	305	2307
80	396	885	4	306	556	·2	354	2631
70	393	860	3	292	635	$\frac{1}{6}$	409	2943
60	391	825	2·5	284	694	·15	447	3150
50	389	772	2	277	774	$\frac{1}{7}$	466	3251
40	383	698	1·5	266	894	$\frac{1}{8}$	528	3558
30	377	597	1	251	1099	$\frac{1}{9}$	599	3866
25	371	526	·9	248	1161	·1	657	4183
20	364	482	·8	245	1234			

II. *Of the Resistance occasioned by Flexure in Pipes or Rivers.*

Mr. Dubuat has made some experiments on the effect of the flexure of a pipe in retarding the motion of the water flowing through it; but they do not appear to be by any means sufficient to authorise the conclusions which he has drawn from them. He directs the squares of the sines of the angles of flexure to be collected into one sum, which, being multiplied by a certain constant coefficient, and by the square of the velocity, is to show the height required for overcoming the resistance. It is, however, easy to see, that such a rule must be fundamentally erroneous, and its coincidence with some experiments merely accidental, since the results afforded by it must vary according to the method of stating the problem, which is entirely arbitrary. Thus it depended only on Mr. Dubuat to consider a pipe bent to an angle of 144° as consisting of a single flexure, as composed of two flexures of 72° each, or of a much greater number of smaller flexures; although the result of the experiment would only agree with the arbitrary division into two parts, which he has adopted. This difficulty is attached to every mode of computing the effect either from the squares of the sines or from the sines themselves; and the only way of avoiding it is to attend merely to the angle of flexure as expressed in degrees. It is natural to suppose, that the effect of the curvature must increase, as the curvature itself increases, and that the retardation must be inversely proportional to the radius of curvature, or very nearly so; and this supposition is sufficiently confirmed by the experiments which Mr. Dubuat has employed in support of a theory so different. It might be expected, that an equal curvature would create a greater resistance in a larger pipe than in a smaller, since the inequality in the motions of the different parts of the fluid is greater; but this circumstance does not seem to have influenced the results of the experiments made with pipes of an inch and of two inches diameter: there must also be some deviation from the general law in cases of very small pipes having a great curvature, but this deviation cannot be determined without further experiments. Of the 25 which Dubuat has made, he has rejected ten as irregular, because they do not agree with his theory: indeed four of them, which were made with a much shorter pipe than the rest, differ so manifestly from them, that they cannot be reconciled: but five others agree sufficiently, as well as all the rest, with the theory which I have here proposed, supposing the resistance to be as the angular flexure, and to increase besides almost in the same proportion as the radius of curvature diminishes, but more nearly as that power of the radius of which the index is $\frac{7}{8}$. Thus if p be the number of degrees subtended at the centre of flexure, and q the radius of curvature of the axis of the pipe in French inches, we shall have $r = \frac{pv^2}{200000\,q}$ nearly, or, more accurately, $r = \frac{\cdot 0000045\,pv^2 q^{\frac{1}{8}}}{q}$. These calculations are compared with the whole of Dubuat's experiments in the following table.

Table of Experiments on the Resistance occasioned by Flexure.

p	q	v^2	r	B.	Y. 1	Y. 2
288	3·22	15030	4·75		6·71	6·98
		11330	3·50		5·06	5·26
		7199	2·33		3·21	3·34
		3510	1·08		1·56	1·62
216		7216	2·49	2·49	2·42	2·52
144			1·50	1·66	1·61	1·67
72			·75	·83	·80	·83
196·5	6·12		1·50	1·66	1·16	1·31
147·4			1·12	1·24	·87	·98
98·3			·75	·83	·58	·65
49·1			·37	·41	·29	·33
112·5	·53		6·00		7·68	6·36
99			5·90		6·74	5·60
288	3·22	3415	1·50	1·55	1·52	1·58
288	3·22	3415	1·50	1·57	1·52	1·58
144			·75	·78	·76	·79
72			·37	·39	·38	·39
196·5	6·12		·75	·78	·55	·62
112·5	·53		1·50		3·63	3·00
720	3·22	5125	5·90	5·90	5·72	5·95
288		3458	1·64	1·59	1·54	1·60
		860	·41	·40	·38	·40
—		821	·39	·38	·37	·38
288	4·10	3148	1·33		1·21	1·30
		7449	2·90		2·59	2·78
294·8 360 112·5	9·9 4·1 1·1		8·64		8·08	8·62

In the last three experiments, the diameter of the pipe was two inches. The radius of curvature is not ascertained within the tenth of an inch, as Dubuat has not mentioned the thickness of the pipes. The mean error of his formula in fifteen experiments, and of mine in twenty, is $\frac{1}{25}$ of the whole.

[To be continued.]

XXI. *Analysis of the Schist that accompanies the Menilite near Paris.* *By Professor* LAMPADIUS*.

THE schist that accompanies the menilite near Paris was formerly confounded with *polierschiefer*, or polishing slate: but Werner has given it the name of *klebschiefer*, or adhesive slate, on account of its property of adhering strongly to the tongue. After his return from France he gave me some of it for the purpose of chemical analysis.

Werner gives the following as its external characters: It adheres strongly to the tongue—colour, pale yellowish gray--without lustre—fracture slaty in even laminæ—opake--takes a slight degree of lustre by scratching—is very tender —separates into leaves spontaneously, which is one of its principal characters—specific gravity under 0·2.

It serves as a gangue to the menilite, with which it is found in the hill of Menil-Montant near Paris.—The following are the results of my chemical experiments on it.

a. Roasted for two hours in a powerful wind furnace, it lost 30 per cent. of its weight. Its colour became a deep brown. It exhibited no signs of fusion, either in a clay crucible, or in a crucible lined with charcoal: yet it had become harder and less friable. That which had been roasted in the clay crucible was rendered very attractable by the magnet.

b. Exposed to the blowpipe on charcoal and with oxygen gas, it melted in a few seconds into an opake glassy bead, of a blackish brown colour.

c. Exposed to the flame of the blowpipe simply, it was not possible to melt it: but with borax a small portion was dissolved, and coloured of a blackish brown.

These preliminary trials, and its effervescence with muriatic acid, led me to suspect that it contained carbonic acid and iron.

d. A thousand parts of the mineral distilled in a retort yielded 270 of carbonic acid.

e. Another thousand parts dissolved in ten times their weight of muriatic acid lost 270 parts.

It

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* Extracted from *Beytrage zur Erweiterung der Chemie*, 1804.

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It contains therefore 27 per cent. of carbonic acid.

The analysis was conducted in the following manner:

1. One part of the mineral in fine powder was put into four parts of concentrated sulphuric acid, in which it dissolved with evident effervescence; and the solution was evaporated to dryness.

2. The residuum was diffused in water, and a gelatinous matter separated, which was still a little yellowish. This was silex.

3. The liquor was filtered.

4. The gelatinous residuum was washed with boiling water, till no further trace of sulphuric acid was discoverable.

5. This water and the filtered liquor were evaporated together, till there remained but ten drachms.

6. Some sulphate of lime separated, which was decomposed by an alkaline carbonate; and after it had been heated and roasted 0·08 of pure lime were obtained.

7. The liquor separated from the sulphate of lime being concentrated by heat, yielded crystals of sulphate of iron and of sulphate of magnesia.

8. I put the whole, without separating the crystals, into a platina crucible, and exposed the saline mass to a strong heat for two hours.

9. After cooling, the mass had an ochrey colour, and a bitter taste. On it I affused boiling water, filtered and washed the residuum.

10. The oxide of iron remained on the filter. After having been dried and roasted it weighed 0·09.

11. I added to the liquor carbonate of ammonia, when a white earth was precipitated, which, dried and roasted, appeared to be magnesia, and weighed 0.28.

12. The yellowish gelatinous residuum (No. 4) was digested in muriatic acid, till its colour became entirely white.

13. Being filtered and washed, the liquor was of the colour of pale white wine. Being precipitated with ammonia, I obtained some more oxide of iron, which, washed and roasted, weighed 0·03.

14. Having redissolved this oxide of iron, and that of No. 10, there yet remained 0·008 of silex.

15. The residuum of No. 13 was found to be pure silex, which, after having been dried and roasted, weighed 0·30.

100 parts of this mineral therefore contain

Magnesia	28
Carbonic acid	27
Silex	30·8
Oxide of iron	11·2
Lime	0·8
Water	0·3
	98·1
Loss	1·9
	100

The most remarkable circumstance is, that this mineral contains no alumine, and includes a large quantity of iron. The outward appearance of the mass would lead us to suspect the former substance, and its light colour by no means indicates so large a quantity of the second. Probably the carbonic acid combining with the oxide of iron conceals its presence.

M. Klaproth, who had before analysed a specimen of this schist, found in it:

Silex	66·5
Alumine	7
Magnesia	1·5
Lime	1·25
Oxide of iron	2·5
Water	19
	97·75
Loss	2·25
	100

XXII. *Comparative Analysis of some Varieties of Steatite, or Talc.* By M. VAUQUELIN*.

THE smoothness and unctuosity of the stones called steatites has been commonly ascribed to the presence of magnesia,

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* From *Annales de Chimie,* tome xlix.

nesia, this earth having been found in all of them that have been analysed; and in consequence all stones possessing these external characters have been classed together. But the pierre de lard, or speckstein, which in some respect may be considered as the prototype of the species, having been analysed by Klaproth, and no magnesia found in it, has changed the opinions of mineralogists on this subject, and led them to wish that some of these substances should be analysed anew,

To remove this uncertainty, M. Haüy gave me three varieties of talc, that I might make a comparative analysis of them.—The first of these is termed in Haüy's Mineralogy laminar talc. It is of a greenish white colour when seen in the mass, very smooth to the touch, and divides into exceedingly thin flexible laminæ of a silvery white.—The second is called in the same work talc glaphique, because it is employed in sculpture; but commonly pierre de lard. It is the bildstein of the Germans. This is compact, very greasy to the touch, and of a colour varying between gray, yellowish, and greenish. Its fracture is dull, uneven, and at the same time scaly.—Of this species M. Haüy sent me two specimens; one of a yellowish white, from a broken Chinese image; and the other of a light rose colour, but in every other respect perfectly similar to the preceding specimen.

Analysis of Flexible Laminar Talc.

1. One hundred parts of this stone, calcined in a strong fire, acquired a yellow colour, with a light rosy tint, was deprived of its flexibility, and lost six parts of its weight. Its laminæ being thus rendered very fragile, I could easily reduce it to powder.

2. The hundred parts thus calcined I heated with twice their weight of caustic potash. The mixture did not melt; but its tumefaction indicated, that a combination between the substances had taken place.

3. The mixture diluted with water was afterwards dissolved in muriatic acid, and evaporated to dryness in a gentle heat. Towards the end of the operation the liquor formed a jelly.

4. The residuum, being lixiviated with distilled water, left a white powder, which, when calcined in a red heat, weighed 62 parts. It was pure silex.

5. Ammonia, mixed with the liquor separated from the silex, formed in it a yellow precipitate of little bulk, from which 1·5 of alumine were separated by means of caustic potash. The remainder was oxide of iron, weighing 3 parts and a half.

6. Having precipitated the iron and alumine by means of ammonia, I put into the liquor a solution of carbonate of soda, and set it to boil. As soon as the mixture began to grow hot, it grew turbid and deposited a large quantity of a white powder, which, when washed and calcined, weighed 27 parts. This substance was magnesia, for with sulphuric acid it formed a salt that had all the characteristics of common sulphate of magnesia.

Flexible laminar talc therefore is compounded of

Silex	62
Magnesia	27
Oxide of iron	3·5
Alumine	1·5
Water	6
	100

From the smallness of the quantity of the iron and alumine, I think these substances may be presumed not to be essential to the formation of the stone; so that perfectly pure laminar talc may be deemed a compound of silex and magnesia.

Analysis of compact rose-coloured Talc.

In the analysis of this variety I pursued the same processes as in that of the preceding; I therefore need not enter into the particulars. The following are its results:

Silex	64
Magnesia	22
Alumine	3
Iron mixed with magnesia ...	5
Water	6
	100

Analysis

Analysis of the yellowish compact Talc (Speckstein).

1. A hundred parts of this stone strongly calcined lost 5 parts.

2. Heated afterwards with twice its weight of potash in a silver crucible no fusion took place, but the matter was greatly increased in bulk, and had become homogeneous.

3. This was diffused in water, and dissolved in muriatic acid. The solution, being evaporated, became gelatinous towards the end of the operation.

4. The matter being dried and washed, a white powder remained, which, after calcination, weighed 56 parts.

5. The silex having been separated by lixiviation, the liquor was mixed with a small quantity of muriatic acid, and ammonia was afterward poured in, which formed in it a copious white flocculent precipitate.

6. The liquor being filtered, the precipitate was washed and dried. This was alumine, and weighed 30 parts. The alumine dissolved entirely in sulphuric acid, and its solution, saturated with the requisite quantity of potash, afforded very pure alum: but the mother water, evaporated afresh, yielded 5½ parts of sulphate of lime crystallized in needles. Thus with the assistance of the alumine the ammonia precipitated the lime from its solution in muriatic acid.

7. The liquor from which the alumine had been separated gave no precipitate with carbonate of soda, even assisted by long boiling. The speckstein therefore contains no magnesia, like the two preceding varieties.—But in recapitulating the products of this analysis we find only 93 parts; namely,

Silex	56
Alumine	29
Lime	2
Iron	1
Water	5
	93

A loss so considerable, which is not common in such analyses carefully executed, led me to suspect that the compact talc contained some other principle, which the processes employed did not enable me to discover. In consequence I treated a hundred parts, reduced to fine powder, with concentrated sulphuric acid.

1. After boiling for two hours I dried the mixture, lixiviated the residuum with distilled water, and boiled the lixivium. At the expiration of a few days I obtained 36 parts of alum crystallized in cubes: and by a second evaporation I procured from the mother water 15 parts more of the same salt mixed with a few needly crystals of sulphate of lime.

2. The stone appearing to me to be but imperfectly decomposed, I powdered it afresh, and treated it as before. On adding the acid employed in this operation to the mother water of the preceding, I obtained 15 parts more of alum, making in all 60 parts. Then, as I employed for this operation very pure sulphuric acid, and added no potash to the solution, it is evident that the stone contained a certain portion of this alkali, and that this substance was the occasion of the loss I had in the first analysis. Sixty parts of alum, however, do not require seven of potash, the quantity of loss, but as the stone is very siliceous, it is probable that the whole of the potash was not extracted by the sulphuric acid, though I boiled the stone twice in it.

The speckstein therefore is composed of

Silex	56
Alumine	29
Lime	2
Iron	1
Water	5
Potash	7
	100

In his analysis of speckstein, M. Klaproth found no potash: but the quantity of water, which, according to him, amounts to 10 per cent., and the loss of 2½, which he experienced, will just balance the deficiency I found. It is probable that M. Klaproth estimated the water by computation, and not by direct experiment; for, to whatever heat I exposed the stone, it never lost more than 5 per cent.

From this analysis it follows, that of the three varieties of talc here mentioned, two only must continue to be so called; namely, the laminar talc, and the compact rose-coloured talc. The third, the speckstein, should be removed to the genus of *alkaliniferous* stones.

It deserves particular remark, that those two varieties, which most resemble each other, and which have always been classed together, should now be separated by analysis: which shows, that minerals should never be classed according to their external appearance, since the most striking analogies in this respect are the most deceitful. In fact, the speckstein and compact rose-coloured talc have the same softness, the same fineness of particles, the same fracture, nearly the same specific gravity; and certainly, if there were any room to suppose that one of the three substances ought to be separated from the talc species, we should be more inclined to suppose it the laminar, than either of the others.

Note. I analysed at the same time that species of talc known by the name of *craie de Briançon*, or French chalk, and I found it to contain the same principles, and nearly in the same proportions, as the laminar talc, and the compact rose-coloured talc. These proportions were,

Silex	61·25
Magnesia	26·25
Water	6
Alumine	1
Oxide of iron	1
Lime	0·75
Loss	3·75
	100

XXIII. *Me-*

XXIII. *Memoir upon the Vineyards and Wines of Champagne in France: Written in answer to certain Queries circulated by M.* CHAPTAL. *By M.* GERMON, *of Epernay.*

[Continued from p. 85.]

XXIII. *What is the Method of operating in the Press, in order to make White Wine?*

THE press being previously well washed and cleaned, and the screw inspected and greased, the fruit is pressed by three successive and rapid turns of the screw in certain districts, and by two only in others, according to the experience of the proprietor, the strength of his machinery, and the expertness of his workmen, or the nature of his grapes. The whole of this operation should be finished in less than an hour by good workmen. Before applying the press, three or four layers of billets or pieces of wood are thrown upon the grapes, placed in such a way as to make the pressure general. After allowing the juice to flow for about five minutes, the press is slackened, in order to stir up the remaining mass, and clear away any obstructions, and the operation is repeated.

The wine flows through a hole into a small tub, called a *carbou,* placed under the press.

When the three pressures have been effected, the wine produced from the juice is called *vin d'elite,* or choice wine. It is called in the language of the workmen *vin de cuevée,* or wine of the tub; but of this expression I highly disapprove, as it gives an idea to strangers that the white wine of Champagne is allowed to ferment (*cuver*) in tubs.

This *vin d'elite* is carried from the *carbou* into a tub adjoining, in which it is allowed to deposit its lees and all other heterogeneous matters during the night: this tub is called the *cuve de depôt.*

After this *vin d'elite* is extracted, there still remains some juice in the husks of the grapes: a new turn therefore is given to the screw of the press, and the wine issues through a hole placed a little lower in the press into another tub:

this juice is called the *first cut* (*premiere taille*), and frequently enters into the composition of the *vin d'elite.* If the wine is not already too vinous, the juice from this last pressure is allowed to flow for about an hour, according to the season or other circumstances.

Another pressure is still given at a subsequent period, and the wine is called *deuxieme taille,* or *vin de tisanne,* so much called for at certain seasons.

A third pressure is sometimes given at another interval, and the wine is muddy, hard, and vinous.

Lastly, a poorer kind of wine, called *vin de rebechage,* is produced by repeatedly pressing the husks until they are perfectly dry: these operations are also called drying the husks.

The *vin d'elite,* after having been allowed to remain all night in the tub, where it deposits its sediment, &c., is put into new or well rinsed puncheons, and the juice from the subsequent pressures is successively treated in the same way.

XXIV. *What Use is made of the Wines last drawn off, which are generally very spirituous; but which, being coloured, cannot be mixed with the first Juices?*

As it has been experienced that the Champagne wines of the last pressures, notwithstanding their vinosity, are too weak, and would occasion too much waste of time and expense to distil them into brandy, it is found more advantageous to sell them in the vineyards of inferior quality, in order to improve the poorer kinds of wine: they are sometimes sold also to inn-keepers, after a sufficient quantity has been retained for the use of the domestics of the proprietor.

In some places, however, these wines are distilled; but it requires from five to eight pieces of them to make one piece of brandy.

[Articles 24, 25, 26, 27, and 28, regard the making of red wine, and will be treated of under a separate head.]

XXIX. *How is Red Wine made?*

The grapes for making red wine are managed with the same precautions as those for white wine.

The only difference consists in loosely depositing the grapes for making red wine in vessels for the purpose: these vessels are covered, and their contents are allowed to remain until the first fermentation has begun in the colouring pellicle of the fruit.

This must, in a state of fermentation, is deposited under the press: the same turns of the screw are given as to the white grapes.

XXX. *How are the White Wines managed until they are fit for drinking?*

The white wine, when left in the state described at the end of No. XXII., enters into fermentation, at first rapidly, and afterwards in a milder manner: when it has gone through all these degrees of fermentation it becomes clear; and when the weather is dry with a clear frost it is racked off, being previously fined with a proper quantity of isinglass. With one pound of Marseilles isinglass forty pieces of wine are fined.

XXXI. *What is the Process of clarifying White Wines; and at what Age are they bottled?*

The isinglass is prepared by breaking it, in order to divide it into small pieces: it is then diluted in some wine drawn from the puncheon. When both are well mixed up together, it is introduced into the bung-hole of the cask, its contents being briskly agitated with a staff or other instrument: the wine is then allowed to rest: it undergoes another slight fermentation, until the coldness of the weather finally settles it.

One month or six weeks afterwards it is again racked off; and a slight proportion of isinglass is added, to bring it to a state of perfect limpidity.

XXXII. *At what Period is it bottled?*

In the month of March these wines are generally bottled.

XXXIII. *How*

XXXIII. *How is the Operation of Bottling performed?*

The wine is drawn off into bottles well chosen, well rinsed, and of an approved manufacture: they are corked with the very best kind of corks: pieces of thread or iron wire are used for fixing down the corks firmly; the bottles are then put into the cellar, and piled up on their sides.

The elaboration of the juice not being completed when the wine is bottled, a slight fermentation takes place in the bottles. About the middle of August in the same year this fermentation begins, and frequently there is a loss by the end of September of five or ten per cent. from the bottles breaking. This loss sometimes goes on increasing until next year, according as the wines are more or less juicy or vinous.

XXXIV. *Is it necessary to cover the Corks with Wax?*

It is not necessary to wax the corks when the wine is bottled: this expense would be thrown away; since about 15 or 18 months after being bottled, when the wine has exhausted all its fermenting principles, and is to be sold and sent off, it must be again disturbed, in order to undergo the operations pointed out in No. 38. This moving of the wine consists of making a slight deposit disappear, which, notwithstanding the first clarification, is indispensable in the different operations necessary: Secondly, such bottles must be filled up as have leaked or lost by filtration through the corks, and the broken bottles are also to be removed.

XXXV. *What are the Faults to which White Wines are subject, either in Casks or Bottles?*

The faults to which white wines are most liable are muddiness (*la graisse*), acidity, and sometimes also yellowness of colour. White wine very rarely becomes muddy when in the casks; but this happens sometimes with bottled wines. The wine is said to be greasy (*gras*) when it is milky and whitish, and when it does not sparkle and present bubbles on its surface when hastily poured out.

When it is ascertained that this accident has happened, care must be taken not to disturb the wine, and the disease generally cures itself by the next or following spring. The whitish sediment turns brown, and deposits or attaches itself to the bottle; and the wine becomes once more diaphanous and sparkling.

XXXVI. *What are the Means used to remedy this?*

When the season has been rainy, the vintage wet, and the juice is too watery, this disease is very frequent; and besides, if the white is in more abundance than the red fruit, the yellow disease is mixed with what is called the greasy, and in this case it is no longer fit for sale: it has a disagreeable taste, and is of the colour of cider: nothing can be done with it, unless it is mixed with common or inferior red wines.

Greasy wines must be cured by time alone; and they very rarely continue more than a year in this state.

All the preservatives recommended in books upon this subject are of no avail: when employed, they are found to injure the quality of the wine instead of improving it.

Note. Acidity being more peculiar to red wines, it will be treated of under that head.

XXXVII. *How does it happen that Half of the Bottles are broken during the first Six Months?*

The breaking of the bottles is owing to several causes more or less direct and more or less well ascertained.

It depends in the first place upon the choice and quality of the wine; the time at which it is put into bottles; the quality of the glass; the nature of the cellar; the temperature of the weather, and even on the way in which the bottles are packed. We cannot therefore assign the exact cause of this accident, so much connected with the phænomena of nature: in general, however, when a proprietor has no more than twenty bottles broken in one hundred he does not complain.

XXXVIII. *When White Wines deposit a Sediment in Bottles, what are the Methods of extracting this Sediment before sending them off to their Place of Destination?*

The sediment in white wines, when they are not spoiled in other respects, is made to disappear in the following way:

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If the wine is not muddy the operation is very simple: it consists in emptying the bottle with care, keeping it in the precise direction in which it lay: the workman with a small hook removes the iron wire which fixes the cork; he then uncorks the bottle, and presents in a perpendicular direction another bottle to it quite empty and well rinsed, and pours out all the wine, leaving the sediment, which, if the bottle has not been shaken, will remain at the bottom.

Some persons make use of a syphon, when the wine is not thick, in order to avoid all shaking.

When the wine is thick the operation is more tedious and more delicate: wooden planks are made use of, in which holes are made at proper distances, in order to receive the bottles: these planks being arranged, adjoining to the collection of bottles, an intelligent and experienced workman carefully takes a bottle from the heap, keeping it in the same position in which it lay: he then gives it a slight shake, and by a regular and long-continued movement he brings into the side of the bottle the sediment which is detached, and, without scattering it through the liquor, makes it slowly descend to the neck: he then places his bottle upon the plank which lies ready to his hand, inclining it in a sloping direction: he afterwards does the same by a second, a third bottle, &c., which he places in the same sloping direction.

Four-and-twenty hours afterwards the workman returns to the plank where he has deposited his bottles; he once more gives them a slight shake, and slopes them a little more, in order to bring the sediment nearer to the cork: if the sediment has then completely fallen down, and the wine is limpid, the workman holds the bottle perpendicularly elevated, and does the same with all the rest of the bottles placed upon the planks: he returns with his hook, uncorks the bottles, and with a dexterous motion of the wrist turns them upside down: the fixed air escapes and pushes out the sediment, which falls into a receiver: the workman then dexterously replaces the bottle upon its end, after allowing nothing to escape, except what is necessary to render it limpid.

pid. Another workman then fills it up with good wine, re-corks it, and the wine is fit for sale.

By this delicate and cautious operation, the wine loses nothing of its briskness, but occasions a great expense in utensils, fresh corks, wire, labour, &c. It has become necessary, however, of late, since the consumption of Champagne has become so general throughout Europe, and great exertions are made to keep up its celebrity.

XXXIX. *Do the sparkling Wines keep well?*

The wines of Champagne, after being put into circulation, and having travelled about, preserve their good qualities for ten years: but when they are kept in cellars, and particularly in those of Champagne, which are superior from the nature of the soil (being dug out of beds of chalk), they will keep for twenty and thirty years.

XL. *What Degree of Temperature is best adapted for the Preservation of Wines?—Point it out with reference to Reaumur's Thermometer.*

I am well convinced that it is by always preserving an equal temperature that the breaking of the bottles may be avoided when in the cellar. Currents of air passing through the cellars should by all means be prevented: but in order to establish an equal current of air, the cellars should be dug very deep: they, however, would be so expensive that few proprietors could be prevailed on to adopt such a regulation. At Rheims, Ay, Hautvillers, Epernay, Cramant, and Vertus, there are, I have seen, some cellars made upon a most excellent plan, and where no expense has been spared.

I have never tried the temperature of the air of the cellars, and I cannot give any results upon this head.

[Articles 41, 42, 43, 44, and 45, being entirely applicable to the management of wines, will form part of a particular treatise upon the subject of red wines.]

XLVI. *What is the Price of an Acre of the best Vineyard Ground?* (The acre being 100 rods and 22 feet.)

	Livres.
At Ay - - - -	6000

Hautvillers

	Livres.
Hautvillers - - - -	3000
Epernay, Pierry, Avise, Cramant	3000
Other vineyards - - -	2000

XLVII. *What does an Acre of the second Quality cost?*

At Ay - - - - -	3000
Hautvillers - - - -	2500
Epernay, Pierry, Avise, Cramant	2000
And the other vineyards -	1000

XLVIII. *What is the Expense of the annual Culture of an Acre of Vineyard, including the Expense of Prunings and of Vintage?*

	Livres.
The ordinary expense of cutting, hoeing, tying, and pruning the vines - - - - -	69
Expense of occasionally propping up such vines as have fallen, &c. - - - - -	60
16 or 18 bundles of props, 50 in each bundle -	30
Dung and carriage of the vines, &c. - -	48
Five empty puncheons for each acre's produce, at ten livres - - - - -	50
Expense of gathering, pressing, keeping the labourers, &c. &c. - - - - -	46
	303

Produce of an Acre of Vineyard.

It is generally understood, that, taking the average of ten vintages, five pieces or puncheons of wine are obtained from every acre.

Three of these are of the first quality, or choice wines: and two of them are ordinary wines.

	Livres.
The three puncheons of best wine may be valued at 150 livres each - - - -	450
The two others at 50 - - - -	100
First result - -	550

From which we must deduct the expense of bottling and of cooperage at ten livres for each piece of the

	Livres.
best wine. The fining of these wines being most expensive	30
For the two other pieces three livres only	6
Annual interest of the money laid out for the ground, &c.	100
Taxes, &c.	72
Labour, &c., as above	308
	516
First result	550
From which deduct as above	516
Net produce of an acre of vineyard in middling years	34

We may easily perceive that the net produce cannot be estimated upon very just and rigorous data, as the wines of Ay, Hautvillers, Epernay, and Pierry, fetch from 200 to 400 livres each piece; and a mean price must be fixed for all the other classes of Champagne wines, which sell for 90 up to 200 livres.

It follows from this statement, that, without great industry, a proprietor can derive but a small profit, who is obliged to sell annually in the cask the produce of his vines: the rich proprietor only, who can afford to put his wines into bottles, and keep them for two or three years, can depend upon a certain and real profit.

In what Manner is the Vine planted in the Mountain?

The vines are planted differently in the mountain and on the banks of the river. The greater part of the vine-growers, who have contracted habits which they will not give up, notwithstanding the inconveniences which they are every day aware of, plant their vines in March only: the shoots they use are either produced from the tall vines which have been beaten, and which have very few roots, or from other plants which spring up among the low vines at the moment of cutting the vines, and which have also very few roots, since they are procured from stalks that have lain on the ground since the commencement of the season.

[To be continued.]

XXIV. *Me-*

XXIV. *Method of painting Linen Cloth in Oil Colours, to be more pliant, durable, and longer impervious to Water, than in the usual Mode. By Mr.* WILLIAM ANDERSON, *of His Majesty's Dock-Yard, Portsmouth**.

SIR,

I BEG leave to lay before the Society of Arts, &c., the following improvements and observations, which I hope will be of service to the public.

Having never heard or read of any method being discovered to prevent paint when laid on canvass from hardening to such a degree as to crack and eventually to break the canvass, and render it unserviceable in a short time; and having been an eye-witness for many years of much canvass perishing for want of such discovery, in the immense quantities painted for covering seamen's hammocks, and for other uses on board his majesty's ships; I long had it under consideration to find out such an ingredient as, when mixed with paint, would preserve the canvass and paint laid thereon from the damages above mentioned: and after experiments for a considerable time, I have discovered such an article, and made trial of it with effect above three years.

The canvass I have painted has been submitted to the inspection of the Navy Board, who are so perfectly satisfied with my new method, that general directions are now given to paint all canvass in his majesty's dock-yards in this manner; which, in addition to the advantages I have before mentioned, actually saves an expense of one guinea in every hundred square yards of canvass so painted, as I have fully stated to them. The ingredient I use is not only serviceable for ships' canvass, but also for canvass designed for paintings, for floor-cloths, and for painted coverings within and without doors. I have no doubt of it being applied to many other purposes I am yet unacquainted with; as, from actual trials of near four years, I can vouch for its being a preservative to red, yellow, and black paints, when ground in oil and put in casks. When the paints were examined at the expiration of such time, they discovered no improper hardness; but when laid on the work with a brush, they dried in a remarkable manner, without the addition of any of the usual drying articles. I still preserve some of these paints for future trials, and I believe this plan of preserving colours will be of essential use to colourmen, and other persons who purchase colours for exportation. The ingredient I use is perfectly simple, being a solution of yellow soap; and the composition for painting is made in the following manner:

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* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1807.——The silver medal of the Society was voted to Mr. Anderson for this communication.

To one pound of soap I add six pints of water in a vessel over the fire; in a few minutes after the boiling of the water the soap will dissolve; whilst hot it is to be mixed with oil paint, prepared as hereafter directed, and is then fit for immediate use. The above quantity of soap solution will be sufficient to mix with one hundred weight of paint. The first coat to be laid upon the canvass is to be entirely of this composition, without first wetting the canvass in the usual way. A very small proportion of it, or none, is necessary in the second coat; and the third coat should be of oil paint alone.

The method heretofore practised in his majesty's dock-yards for painting canvass, was as follows: The canvass was first wet with water, then primed with Spanish brown; a second coat given it of a chocolate colour, made from Spanish brown and black paint; and, lastly, finished with black. This mode is destructive, and more expensive than mine in the proportion before mentioned. In my method, to ninety-six pounds of English ochre ground in boiled oil, I add sixteen pounds of black paint, being one-sixth in proportion of the ochre; this, when mixed, forms an indifferent black. The solution, made of one pound of soap and six pints of water, is to be added to this paint, and well united therewith; and without the canvass being previously wet, this composition is to be laid upon the canvass as stiff as can conveniently be done with the brush, and this first coat will form a tolerably smooth surface. The second coat is to be formed of the same proportion of English ochre and black, without any soap solution; and the third or finishing coat, to be done with black paint as usual.

I am, sir, your obedient humble servant,

WM. ANDERSON,
Master Painter of H. M. Dock-Yard
at Portsmouth.

Portsea, Oct. 31, 1806.

SIR,

AGREEABLY to the request in your letter, I have enclosed certificates relative to my new method of painting canvass; and I take the liberty of informing you of a method of obtaining from painted canvass, unserviceable, the whole of the colour laid thereon, and to do it at a very small expense. This I discovered since I last wrote to you, and I believe it will be of considerable advantage to government, who, for want of such a thought, have buried and burnt immense quantities of ships' hammock cloths, when found unserviceable, to prevent embezzlement from taking place. I suggested the idea to N. Diddems, esq., builder of Portsmouth yard, who communicated it to the hononrable George Grey, commissioner. I obtained leave to make an experiment, which I repeated thrice, and found that from one ton of painted canvass, unserviceable, I obtained, upon an average, four hundred weight of dry colour, in value to government nine pounds six shillings; the expense of the process not exceeding six shillings.

This I effected by calcination, raking aside the ashes and sprinkling them with water, to prevent loss of paint through excess of heat. By passing the calcined matter through a fine sieve, it is perfectly prepared for grinding; it grinds well, possesses a good body for covering with, and dries well with a good gloss. Its increase of bulk, in comparison with common colour of equal weight, gives it the advantage of covering more work. The colours yielded by the calcination of different coloured canvass are as follow: viz. Canvass which has been painted with black paint only, produces a black colour. Canvass finished black, but which has had a previous red or yellow ground, will produce a dark chocolate colour. Canvass painted lead-colour will yield a good dark lead-colour.

I am, sir, your obedient humble servant,
WM. ANDERSON.

Portsea, March 25, 1807.
To C. TAYLOR, M.D. Sec.

Certificates, dated March, 1807, were received from the following persons, viz.

A. STOW, lieutenant and commander of the gun-brig Steady, stating, that in the preceding month of October he had received on board his ship a set of hammock cloths, painted after the method invented by Mr. William Anderson, which had been constantly in use since the time above mentioned, and appeared fully to answer the end proposed, of rendering the canvass soft and pliable, of preventing its cracking, or the paint peeling off, and which in the old method had been a subject of much complaint.

JOHN PRIDY, lieutenant and commander of the Gladiator, and formerly commander of the Dapper, on which latter ship a set of hammock cloths, painted after Mr. Anderson's method, appeared fully to answer the end proposed.

P. F. WYATT, oil- and colour man, Portsea, stating that he had seen canvass painted after Mr. Anderson's new method, which, after a trial of sixteen months, remained perfectly soft and pliable, the paint by no means cracking or peeling off, and that the gloss was retained, though it had been exposed to all weathers. He further added, that he had seen the paint prepared by him from old painted canvass found unserviceable, and had worked and painted therewith; that it was, in his judgment, very good, and would answer either on canvass, wood, or iron.

Ns. DIDDEMS, master shipwright, Portsmouth dock-yard, stating, that Mr. Anderson had proposed to him to obtain, by calcination, from old unserviceable painted canvass, the paint which had been laid thereon; that such experiment was made, and four hundred weight of dry serviceable paint prepared from one ton of such canvass; that he had seen it when ground in oil and laid on work, when it appeared to possess all the properties of good paint, and had therefore been recommended by him to the Navy Board.

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SIR,

In answer to your letter of the 25th of April, in which you informed me that the committee were desirous that I should furnish them with a sample of canvass painted in the old method, and another on my improved plan, I trust that I shall be able fully to comply with their request. In the first place, I have sent a small sample of the residuum of the burnt canvass, fit for grinding in oil for paint, also a piece of canvass painted therewith, marked No. 1; another piece painted after the old method, marked No. 2; another piece painted according to my process, marked No. 3; and, lastly, a piece finished entirely with a new composition, marked No. 4; each sample having received three coats of paint. Upon examining No. 2, you will find it becoming from time to time more stubborn, in consequence of the paint hardening; and when a small ridge is formed in it, by pressing it between the finger and thumb, it will soon discover that it is subject to crack, and by this means, permitting the wet to enter it, will soon rot the canvass.

The space of time proper between laying on the new preparation and the second coat, ought to be one entire day; but if saving time is an object, the second coat may be put on the day following the first; for, if the canvass is placed in an advantageous situation for drying, the composition will dry or harden so as not to rub off.

Canvass finished entirely with the composition, leaving it to dry one day between each coat, will not stick together if laid in quantities, as you will find by making experiments on the sample No. 4.

Since the Navy Board have given directions for ships' canvass to be painted according to my method, I find, upon calculation, that I have painted upwards of twenty thousand yards since November last, a great part of which has not been hung up for painting and drying more than one week, as no more time could be allowed me, in consequence of ships sailing. My plan was therefore to lay on the composition the first day, to coat it the second day, and, leaving one intermediate day, to finish it on the fourth. Three days were then allowed it to dry and harden; and when afterwards taken down and folded together in cloths, containing sixty or seventy yards, they did not stick together.

Having no means of giving information to persons concerned in grinding colours, so well as through the medium of the Society of Arts, &c. I beg leave further to relate how I have, for the last three years, saved the labour of three men out of four in grinding colours with the common mills employed for that purpose. One mill has ever been considered sufficient for a man to turn, whereas one man can now, with perfect ease, turn four mills; this is effected by placing two mills on each side of the winch, so close as only to leave room for the fly wheel to play between them. The spindles of each on either side are locked together by a small iron collar, with a pin passing through it. The distance of the mills thus paired from each other, in order for the man's standing between them to turn, is two feet six inches. The distance of the arms of the winch screwed on the end of the spindles on either side, is two feet two inches; the length of the arm is one foot six inches from the spindles to the bar across which the man clasps in order to turn.

Fly wheels at the extremity are impediments. Necessity was truly the mother of invention to me in this case, as I had great demand for paint, and I was not allowed men sufficient for the work in the common way.

Persons will scarcely believe, without seeing the experiment, the ease with which they turn. If a little extraordinary motion is first given them, and they are then left alone, they will continue to go round sixteen times; so that a man with one hand may turn them.

I am, sir, your obedient humble servant,

WM. ANDERSON.

Portsea, May 6, 1807.

To C. TAYLOR, M.D. Sec.

SIR,

I HAVE stated to the Admiralty Board the several improvements made by me in paint work; and in consequence thereof they have desired the principal officers of our yard to report to them on their merits. The officers, who have for more than twelve months past daily had the execution of them under their inspection, have recommended the same in stronger terms, and the advantages thereof, to the lords commissioners, beyond my statement. I have enclosed to you a certificate relative to the ship Hibernia, which arrived here the 12th of May last, and for which vessel I painted a set of hammock cloths, containing thirteen hundred yards of canvass, in June 1806, after my new method.

I am, sir, your obedient humble servant,

WM. ANDERSON.

Portsmouth, Nov. 27, 1807.

To C. TAYLOR, M.D. Sec.

XXV. *Experiments on various Earths, undertaken with the View of ascertaining whether they are metallic Oxides.*

By DAVID MUSHET, *Esq.*

THE late interesting experiments of Mr. Davy in metallizing soda and potash, have brought to my recollection a train of experiments in which I was engaged eight years ago, with a view to metallize some of the earths. Though considerably disappointed in my first attempts, yet I have repeatedly returned to the charge with increased hopes, but without obtaining any thing like a perfect result.

In giving to the public a detail of my experiments, it is impossible that I should mean to bring them forward with any view to a comparison with the perfectly original and satisfactory results of Mr. Davy. Our modes of operating were so totally different, that similar results could not be expected. Should, however, any beneficial or useful purpose arise from the knowledge of the new metals; then, so far as a simple mode of operation goes, my reasonings and practice may be of service to others who may engage in a similar undertaking.

My first experiments were made with pure earths, clay, silex, lime, barytes, and strontian, considering them as metallic oxides, whose oxygen might be carried off by presenting them with carbon at a high temperature, and secured from the access of air. This reasoning I carried into practice by cementation for hours, and sometimes for three or four days. Various earths were exposed imbedded in finely-pounded charcoal. These were afterwards freed from the carbonaceous matter, and exposed to fusion in high heats in a wind furnace. Clay and silex I found infusible under the highest heat that could be urged. Barytes, lime, and strontian, were fused with various proportions of charcoal, but no result occurred from which any conclusion could be drawn favourable to the idea of either a part, or the whole, of the oxygen having been removed from the respective earths, nor was it found that any loss of weight took place, (as is the case with iron ores,) which would not have occurred by simply exposing these substances to the same temperature. The glasses resulting from the different fusions were various in colour, whitish, opaque, brownish, and black. The only circumstance which indicated change was in the barytes, the different fusions of which always gave a thin pellicle on the surface that never was resolved to glass, but was always strongly alkaline. This, at the time, I could not account for, nor till Mr. Davy's discoveries were announced. The probability then appeared, that this was a portion of the barytium, which, during the operation, had been metallized, but, in cooling, had again attracted oxygen from the atmospheric air, and had passed into the state of an alkaline earth.

After many experiments, I at that time abandoned the pursuit, and arranged those specimens of glass which appeared most fit for future examination, should the subject present itself under any new shape. Some years afterwards, having occasion otherwise to examine the boxes in which these specimens were kept, I was much surprised to find, that many of the glasses had become converted into a fine powder. I was induced, from a similar circumstance having taken place with a glass of manganese, to infer, that in the original experiment a de-oxidation had taken place, and that by the reassumption of oxygen the present effect had been produced.

I then thought of pursuing some mode of operation which would enable me to detect what proportions of oxygen were united to the various earths. This I thought of accomplishing by a set of comparative experiments in the fusing of pure malleable iron with the different earths. Iron being a highly oxidable metal, the quantities disappearing would indicate the comparative quantities of oxygen in each of the earths. This was with a view to form the most ample data for subsequent experiments, and to compare the alkalis with alkaline earths. These experiments embraced a number of substances, as will be brought forward in the detail.

I.—200 grains of calcareous earth (very pure Paris white), deprived of its carbonic acid, were mixed with 50 grains of iron filings: these were mixed together and exposed to a high degree of heat; a perfect fusion of the earth had taken place, which was now converted into a black glass of a deep jetty lustre. Two small but finely polished spherules of metal were obtained weighing 12 grains—loss by oxidation 38 grains, or 76 per cent. It may be proper to state here, that 50 grains of malleable iron filings, the same used in this and the following experiments, were fused *per se* in ten minutes, and the button formed in consequence weighed 47½ grains—loss only 7½ per cent.—oxidation in consequence of the calcareous earth nearly 70 per cent. more.

II.—100 grains of pure barytes and 50 grains of filings of malleable iron were fused together. A black brownish opaque glass was obtained, and beneath a smooth-skinned metallic spherule was found weighing 9½ grains—loss 40½ grains—equal to 81 per cent. From the portion of the earth to the iron in this experiment being double, in place of quadruple, as in the last experiment, it was inferred that 100 grains of calcareous earth would oxidate 19 grains of iron, whereas 100 grains of barytes in this experiment oxidated 41½ grains.

III.—100 grains of carbonate of barytes in a similar experiment oxidated only 41½ grains of iron *.

IV.—100 grains of carbonate of strontian deprived of its carbonic acid oxidated 21½ grains of iron.

V.—100 grains of potash oxidated 28 grains of iron.

VI.—100 grains of salt of tartar oxidated 22½ grains of iron.

VII.—100 grains of calcined borax oxidated 3½ grains of iron.

VIII.—100 grains of window glass, composed of two parts of soda and two of Lynn-sand, oxidated 4¼ grains.

IX.—100 grains of bottle glass oxidated 3 grains of iron.

X.—100 grains of roasted ironstone, containing iron 48·5; oxygen 15·5; earths 36·=100, oxidated 33 grains.

XI.—100 grains of manganese in a similar experiment oxidated 25¼ grains.

It appeared from these experiments, that either barytes contained the greatest dose of the oxidable principle, or that it gave it out to iron with the greatest facility. It was therefore fixed upon as the most proper subject for further experiment, the details of which I shall state as shortly as possible.

XII.—100 grains of pure barytes were exposed to a heat of 168° of Wedgwood. An emerald-coloured glass was obtained, which, in cooling, arranged itself into numerous small squares; the surface was covered with a crust or pellicle very like an oxide of nickel.

XIII.—100 grains of barytes mixed with 10 grains of charcoal were exposed to the same heat. The result was a dark-green glass, accompanied with a similar saline crust, rather more of a coppery colour. The charcoal had disappeared.

XIV.—The same experiment was repeated with 20 grains of charcoal. The fusion, though exposed to as high a heat, was less perfect. A greater quantity of the apparent oxide was formed, and a proportionably less quantity of glass.

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* The result of this experiment being the same as Experiment II., Mr. M. has probably made a mistake in copying his notes.—Edit.

XV.—This experiment was performed with 100 grains of pure barytes, 200 grains of iron-stone, and 8 grains of charcoal.

The reasoning which suggested the use of iron-stone proceeded upon the supposition that the surface incrustation was an oxide of barytes (barytium), effected to a certain stage of purity, in consequence of fusing pure barytes with charcoal. It seemed probable that the addition of a second affinity would, with the acid of the charcoal, tend to remove the more fixed and ultimate portions of oxygen over which the charcoal had no power. It was conceived that the iron-stone, not being saturated with oxygen, might withdraw a portion of that supposed to exist in the partially revived barytes, and tend to metallize the result. Malleable iron, as in the other experiments, might have been used; but as this would always have entailed the presence of a button of iron, the result, it was supposed, would be attended with some uncertainty. In the present experiment it was thought proper to reduce the quantity of charcoal to eight grains, lest any part of the iron contained in the oxide might thereby be revived.

This experiment being exposed to a similar heat as the others, a flat blackish mass was obtained weighing 270 grains—loss of weight in the whole 38 grains. The iron-stone alone ought to have lost 70 grains. It was therefore inferred, that some new combination had taken place, and what in other experiments would have been volatilized, in this, became fixed. When the mass was divided, it exhibited an uncommon appearance: the surface was covered with a black de-vitrified glass; the fracture showed a beautiful metallic crystallization and brilliancy, with some large metallic plates not unlike carburet of iron. Toward the lower edges of the button the crystallization was very perfect. Although this mass had all the beauty and splendour of a metallic regulus, yet there was a great deficiency of metalline property: it was eagerly brittle, and easily reduced to a powder; exhibited little or no lustre in grain, or when scratched with a knife point. This experiment was repeated under various temperatures, but without being more successful in producing the metal in a state of greater purity.

XVI.—100 grains of pure baryes, 200 of iron-stone, and 10 of charcoal, fused together, gave the following result: Surface a black shining glass of considerable thickness, covering a perfect crystallized regulus of the same matter as was found in Experiment XV. The same want of metallic property was evident in this as in the last, though the specific gravity of the mass was very much increased. On one side (and not under the imperfect regulus of barytium) was found a metallic spherule, supposed to be revived by the addition of the two grains of charcoal—it weighed 1½ grain.

XVII.—100 grains of pure barytes, 200 of iron-stone, and 13½ of charcoal, gave a result similar to the former in point of glass. A smaller and more perfect regulus was found under it, alongside of which, as in the former, was found a metallic spherule of iron weighing 10 grains.

XVIII.—100 grains of pure barytes, with 200 of iron-stone, and 20 of charcoal. The result of the fusion of this compound presented something different from any of the former. A button of iron was found weighing 33 grains; this was surmounted by a black glass, which now contained no barytium. Over this glass, and immediately on the surface, a metallic crust presented itself. It was crystallized in small concentric radii inclining to a brownish silvery colour, and brightened a little under the file. It had, in every respect, a more metalline appearance, and, so far as polish, continuity of grain and lustre were concerned, was much superior to any of the former results.

The same experiment was repeated again and again, sometimes with increased doses of iron-stone and charcoal; but none of the results were more perfect than the present. Despairing, therefore, of any thing more perfect with iron oxide and charcoal, it was resolved to try the effect of malleable iron filings in place of iron oxide. Having used all the pure barytes in my possession, the following experiments were made with a fine crystallized specimen of carbonate of barytes.

XIX.—115 grains of this carbonate were exposed for two hours to a high red heat, and came out unchanged, both as to weight and appearance.

XX.—40 grains pounded small were exposed to a high white heat in a Cornwall clay crucible. A rough whitish mass was obtained, which evidently had been fused—weight 36 grains. Loss 4 grains, supposed to be carbonic acid. It was remarkable that the present result in cooling underwent several shades of colour chiefly metallic; a green succeeded by a vivid purple was the most decided.

XXI.—90 grains of this same carbonate, pounded small, and 45 grains of iron filings were mixed together and fused: the upper surface of the result was covered with a brownish silvery enamel regularly crystallized in small stars, each radiating from a common centre. The thickness of this metallic crust was nearly one-sixteenth of an inch. Its fracture presented brilliant crystallized facets of a highly metallic appearance. Under the barytium was found a jet black shining glass, in which was inserted a smooth button of iron weighing 20 grains. Beneath this button was another layer of barytium regularly crystallized, but of a less metallic appearance than the upper stratum. In this experiment 90 grains of carbonate of barytes had furnished the means of oxidating 25 grains of iron.

XXII.—50 grains of carbonate of barytes, and an equal quantity of iron filings, gave by fusion a double stratum of barytium. Interposed between was a layer of black glass; the crystallization and brilliancy of the upper stratum and the general appearance of the whole were very similar to the last experiment. The button of iron found in this result weighed 33 grains. Loss 17 grains oxidated by 50 grains of carbonate of barytes.

This experiment was repeated with various proportions of the carbonate of barytes and the iron, and with greater quantities of matter, yet none of them were so perfect as those detailed in the foregoing experiments. Greater quantities of the apparent regulus were obtained, all tending to the same crystalline arrangement, but inferior in point of colour and brilliancy.

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I next varied the experiments in the following manner: Iron ore was de-oxygenated almost to a state of metallic purity. The ore in this state consisted of 90 parts of iron, with which a little oxygen was still combined, and 10 parts of calcareous earth.

XXIII.—280 grains of this ore, and 280 grains of carbonate of barytes were fused together, and the result was as follows: A double stratum of the reguline matter was formed; the upper evidently more metallic than that below; yet the general appearance of this was less metallic than the results of Exper. XXI and XXII. The button of iron was covered on its surface with a silvery-white circle delicately crystallized in the starry form peculiar to the barytium; and this being the first crystallization of that form I had ever seen on iron, I was led to infer that an alloy had taken place between the iron and the metal of the barytes. The weight of this button was found to be 172 grains. Loss 108 grains of matter, which, taken at the rate of 90 per cent. of metal in the ore, gives the quantity of iron oxidated by 280 grains of barytes $97\frac{2}{10}$ grains, or $34\frac{3}{10}$ grains of iron from 100 grains of carbonate of barytes, a result but little different from the last experiment.

Similar experiments were performed with various proportions of the barytes and de-oxygenated ore; and corresponding results were obtained. When the iron existed in equal portions to the barytes, a quantity of crystallized regulus was obtained nearly equal to one half of the result—the other half being always a black glass. When a greater proportion of iron filings was used, and to the extent of two parts to one of the barytes, a greater proportion of the regulus was obtained; but then the experiment was difficult to manage, from the great heat necessary to fuse so large a relative proportion of malleable iron, without destroying the barytes altogether. On the contrary, when the carbonate of barytes or pure barytes were used to the extent of two parts to one of iron filings or oxidated iron, the mass was chiefly resolved into a glass, and the greatest part of the iron was oxidated: the quantity of regulus small, and a perfect result always precarious, from the violent action of the barytes upon the clay of the crucible. In this respect barytes resembles some rich oxides of iron, which are totally unconfinable in a clay crucible at a high temperature.

After making two hundred experiments without having obtained, what I thought, a perfect globule or regulus of pure metal, I abandoned the subject till new reasonings and after-reflection should point out any new tract which was likely to lead to more success. I was satisfied that I had obtained an approach to metal, and was even convinced that the metal at one part of the operation was more decidedly so than it afterwards appeared to be when examined cold: but I was not at all satisfied that the regulus I had obtained was in its ultimate state of purity.

Disappointed in my hopes of success with barytes, my experiments on lime and strontian were few; but limited as they were, I was convinced that they were similar compounds, and capable of decomposition. I did not succeed in obtaining so compact reguli as with the barytes, but both of them showed metallic crystallization upon the surface, although apparently more volatile and destructible than those of barytes.

Should I at any future time increase my experiments on these substances, and should the results point to any thing new and likely to be beneficial, I shall communicate them. I am confident that an increased knowledge on the subject of lime-stone will prove highly interesting to the manufacturer of iron. The single circumstance of its being a metallic substance combined with oxygen, and as such acting its part in the operations of the smelting furnace, will enable him to explain facts that cannot be reconciled to any past reasoning or knowledge on the subject.

In regard to silex or clay, considered as metallic oxides, I have been able to ascertain nothing decisive. They seem not (judging from the experiments I have made) in the most distant manner allied to the other three earths, though they may be more akin to each other. It is possible that silex may prove to be clay completely deprived (or nearly so) of all its moisture. Or, in other words, that clay, by fire or other natural processes, becomes oxygenated to such an extent as to convert it into what we call silex. May clay not prove to be water and oxygen, and silex this oxygen without water?

XXVI. *Proposal for altering the Scale of the Thermometer.* *By* RICHARD WALKER, *Esq., Oxford.*

To MR. TILLOCH,—SIR,

I BEG leave to announce, through the medium of your useful Miscellany, an intention I have of offering to the public notice an alteration in the scale of the thermometer, which many of my friends, as well as myself, have adopted, from a persuasion of its being founded on the truest principle.

The alteration I shall suggest, and which presented itself during the long course of my thermometrical experiments, I shall only briefly state now, reserving a fuller account of the reasons which induced me to adopt the scale I now propose to another opportunity.

The two fixed points, viz., the freezing and boiling points of water, as they have hitherto been, will probably never fail to be continued, as being perfectly sufficient for the accurate adjustment of thermometers.

The commencement of the scale, and the number of divisions, only appear to claim attention. With respect to the first, since neither of the extremes of heat or cold (to speak familiarly) are likely to be ascertained, the hope of fixing 0 at either of these may be entirely relinquished, and it remains to fix 0 at the fittest intermediate point.

Hence I presume to propose the following mode of graduation, stating briefly the principle on which I proceeded. Having ascertained that the temperature of 62° of Fahrenheit is the temperature at which the human body in health is conscious of no inconvenience from heat or cold, and that a deviation from that point of only one or two degrees, above or below, actually produces that effect, under ordinary circumstances, I fixed my zero or 0 there.

With respect to the divisions, I adopted those of Fahrenheit, from an opinion of that being the fittest, consider-

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ing those of Reaumur, the centigrades, &c., as being too few, and decimal divisions unnecessary in a thermometrical scale.

Hence it will follow, that 0 being placed at 62° of Fahrenheit, 150° will be the boiling, and minus 30°, the freezing points of water; and all other points on Fahrenheit's scale may be reduced to this, by subtracting 62 for any degree *above* 0 of Fahrenheit; and adding 62 for any degree *below* 0.

I shall only add, at present, that there is a very convenient mechanical mode of adjusting this scale in the construction of thermometers.

For ordinary meteorological purposes, a scale of this kind extending to 65 degrees above 0, and as many degrees below 0, will be sufficient.

Rd. Walker.

Queen-Street, Oxford,
Feb. 17, 1809.

XXVII. *On the Difference between the Products obtained by Distillation of recent and of dried Vegetables.* By *Mr.* Garden, *of Old Compton Street, London*[*].

That most recent vegetable bodies during the process of desiccation undergo a material change in their external appearances becomes evident from mere inspection; but that an alteration frequently takes place in their physical properties, and also among their constituent principles, by that process, has not, perhaps, in every case, been so clearly established.

Our knowledge indeed of the physical properties of vegetable substances, obtained from an acquaintance with their chemical composition, has hitherto made but little progress; arising, no doubt, from the exceeding alterability of their nature, when subjected to those processes usually employed for disuniting their component parts; some of their ingredients being too volatile to be retained, while others become so modified by the action of moderate temperatures, as to render it difficult to trace the precise order of union which those elements maintained in the original compound. Thus it is found that vegetables, both of the noxious and esculent kind, yield by that species of chemical decomposition which is effected by fire, the same common elementary principles; whence it follows, that the precise nature of a vegetable cannot be determined by the mere knowledge of its constituent parts.

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[*] Communicated by Mr. Garden.

When the recent leaves of vegetables are exposed to a degree of heat but little exceeding the medium temperature of our climate, an evident change is quickly produced; their bulk becomes greatly diminished, their colour less vivid; the fragrance, if they possessed any, is in most cases considerably impaired, and in some instances totally destroyed: it will also be found that a considerable loss of weight has been sustained.

These obvious changes may chiefly be referred to the evaporation of aqueous moisture, dissipation of the aroma, and loss of a portion of the volatile or essential oil. That this last effect does not take place is an opinion entertained by some practical operators, who maintain, that from those vegetables containing essential oil, the greatest quantity may be obtained by previous drying before they are submitted to the process of distillation.

It is not my intention either to combat this opinion or to enter into a detail respecting the chemical composition of vegetable bodies, but simply to state the following fact, which has lately come under my observation.

A quantity of the dried leaves of peppermint (*mentha piperita*), which had been included in casks well closed for nearly two years, and apparently in a state of good preservation, were exposed to distillation, with a view to obtain the essential oil. The operation was conducted in a common still furnished with a connecting refrigeratory, and the products received in an Italian recipient, such as is usually employed for the separation of the lighter essential oils. After a considerable quantity of the fluid had distilled, and that which remained tasted but little of the plant, the process was discontinued. On examining the receiver, it was observed, that the produce of essential oil was excessively small, notwithstanding the quantity of leaves which had been operated upon amounted nearly to 40 pounds.

Apprehending some mismanagement in the operation, another quantity similar to the preceding was introduced into the still; attending at the same time to every circumstance which could possibly be imagined to facilitate the development of the oil. A piece of basket-work was placed in the bottom of the still, and a quantity of water was added sufficient to give fluidity to the mass, and prevent the possibility of empyreuma taking place; added to these, the boiling point was attended to with care, and the first portions of the distilling liquid were suffered only to drop slowly from the condensing vessel.

In this last operation, however, the value of the products did not differ materially from that of the preceding; and the result of several subsequent distillations appeared to show that the essential oil could not be obtained from the dried plant in a proportion equal to that afforded when in a recent state.

In the respective operations it was observed, that although the produce of oil was comparatively inconsiderable, its absence seemed to be balanced by an abundant produce of water highly impregnated, both with the taste and flavour of the plant.

This circumstance appears to warrant the presumption, that the herb, notwithstanding the length of time which it had been enclosed in the casks, had not suffered so much deterioration as the diminished produce of oil might seem to indicate; for although the essential oil did not appear in an uncombined state, its particles may have undergone some peculiar modification, in virtue of which it was rendered more soluble, and hence the increased quantity of highly impregnated water may be accounted for.

The addition of alkaline substances in small proportions has been supposed by some to accelerate the evolution of volatile oils from their bases: in the present case this expedient was not productive of any remarkable effect.

XXVIII. *Report on a Manuscript Work of M.* ANDRE, *formerly known under the Name of* P. CHRYSOLOGUE DE GY, *entitled* A Theory of the actual Surface of the Earth. *By MM.* HAUY, LEVIERRE, *and* CUVIER. *Read to the Class of Mathematical and Physical Sciences in the National Institute**.

As this is the first opportunity which has hitherto presented itself of entertaining the class with geological subjects, it will not perhaps be considered foreign to our purpose to make some general reflections on the manner in which a society such as ours may and ought to consider this kind of researches.

The natural history of unorganized bodies, commonly called crude matter, or minerals, is divided into two principal branches. In the one we examine each of these bodies by itself, and in its physical and chemical properties, and hence determine its distinctive characters, and its rank in the general system. This part has more particularly retained the name of *mineralogy*, which has almost always been cultivated by men of talents, and has now attained a degree of precision and exactness, equal, at least, to that of all the other physical sciences.

The object of the other branch of the history of minerals is the reciprocal position of their different species, and of the masses composed of one or more of these species. It is this branch which teaches us what materials constitute the vast extent of countries, what others are confined to vacancies, crevices, and fissures of the preceding; it shows us what substances respectively form the great chains, inferior mountains, ridges, and plains; it is especially occupied with the super-position of minerals, and enables us to distinguish between those which always bear, and those which always surmount others, or, in a word, to discover the order of the different strata. To this branch is given the appellation of *geology*, *geognosy*, or *physical geography*, according to the extent and profundity of the researches.

It is evidently a science susceptible of as much accuracy as mineralogy properly so called. To give it this quality,

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* From Transactions of the Institute, 1808.

It is only necessary to treat it as all the natural sciences ought to be; that is to say, to collect with care the particular facts, and to deduce no general conclusions until these facts are collected in sufficient numbers, observing always the rigid rules of logic.

It is also evident that this science constitutes a part of natural history not less indispenable to the knowledge of the globe than mineralogy itself. It is to the latter, what the history of the climate, soil, and situation proper to each plant is to botany. Its utility to society, if it were once completed, would be no less evident. By it we direct our researches for divers minerals, and by the same means anticipate the difficulties and expenses attending numerous works, which could not otherwise be known but by experience. Thus, our engineers could not calculate the expense of a subterraneous conduit to substitute for the machine at Marly: geology taught them that at this place they could expect to find nothing but chalk.

The miners, who are more interested than any other artists to possess this kind of knowledge, have made it a particular study, according to the class of minerals in which they are engaged. They have determined the characters of mountains with metallic veins, and know perfectly the countries where there is nothing to be found, and those where something may be gained. But from the very nature of the motives by which they are influenced, they have almost entirely neglected to examine districts poor in metals. It is thus that in our vicinity each workman knows but the kind of quarries in which he works. He who seeks plaister of Paris neither knows what is above nor what is below the strata of gypsum: the quarrier is ignorant that under him is potter's clay, &c.

He who is the least acquainted with science, will feel that a study which furnishes data with regard to all the useful minerals, similar to those of the miners on metallic veins, must be of the greatest importance to society; and that were it extended to all the known minerals, it would form an equally agreeable and curious branch of natural philosophy.

It is probable that we should have principally studied, with this view, the surface of the globe, and the trifling portion of its interior where we are able to penetrate it, if there had not been found minerals entirely crude. As these minerals must have been originally disposed in some order, we should not at first have seen in their disposition proofs of successive action and of revolutions, if a very great part of their beds had not been replete with the remains of organized bodies. The fossils and petrifactions indeed, by exciting curiosity and arousing the imagination, have given a too rapid impulse to geology, have raised it too superficially above its first basis, which should be founded on facts, and carried it to search for causes which should have been its final result. In a word, from a science of facts and observations it has changed into a tissue of hypotheses and conjectures, so vain and so contradictory that it is become almost impossible to pronounce its name without a smile.

At first fossils and petrifications were considered as *lusus naturæ*, without considering what it really meant. But when a more profound study had shown that their general forms, their texture, and in many cases their chemical composition, were the same as those of analogous parts in living bodies, it became necessary to admit that these objects had also possessed life, and that consequently they had existed at the surface of the earth, or in the waters of the sea. How did they become buried under immense masses of stones and earth? How were marine bodies transported to the summits of mountains? But above all, How was the order of the climates reversed, so that we find the productions of the torrid zone near the pole?

When it was perceived that almost the whole surface of the globe was thus covered, the general and powerful causes which had so dispersed them began to be considered. Genesis, and the traditions of almost all Heathen nations, offered one, to which it was natural that philosophers should first have recourse: it was the deluge. The petrifications passed as proofs of it; and during nearly a century the works on geology consisted either of efforts to find the physical causes of this great catastrophe, or to deduce from it as an effect the actual state of the surface of the globe. Their authors forgot that the deluge is stated in Genesis as a miracle, or as an immediate act of the Creator's will, and consequently that it is superfluous to seek any secondary causes.

[To be continued.]

XXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

FEBRUARY 2.—The reading of Mr. Troughton's paper on the Division of Mathematical Instruments by ocular Inspection was concluded. One of the methods adopted was the use of a roller one-sixteenth the diameter of the circle to be divided. The description of this principal instrument the author has deferred till a future communication.

A most curious and interesting paper by Mr. Davy was read, giving an account of various experiments on the action of potassium on ammonia, from which it appears, that a considerable quantity of nitrogen can be made to disappear, and can be regenerated. When it disappears, nothing is obtained in its place but oxygen and hydrogen; and when it is *formed*, its elementary matter is furnished by water.

There seem to be, at present, only two modes of explaining these extraordinary and entirely unlooked-for results: *i. e.* that nitrogen is either a compound of hydrogen and oxygen,—or, which is the most probable, that hydrogen, nitrogen, ammonia, water, and the nitrous compound, all contain the same *ponderable* elementary matter, and that their different forms depend upon different electrical states. The paper concluded by stating that the author was still pursuing this inquiry, so intimately connected with the whole arrangements of chemistry and meteorolgy.

Feb. 9.—Dr. Young furnished a series of numerical tables of the elective attraction of acids with alkalis, by means of which 100 figures are made to represent the affinities of 100 different salts, which would otherwise require above 5000 words to express.

Feb. 16.—A paper by Mr. Brodie, describing a twin foetus, fœtus, nearly the full size, seven months old, and without either heart, liver, or gall-bladder, was read. This was considered the best formed fœtus which has hitherto been known without a heart, although the author cited a considerable number. It appears that all such children have been twins, and that the present was quite as large as the other fœtus which had its organs complete.

Capt. Burney furnished two papers, one on the motion of heavy bodies in the Thames, detailing some experiments with loaded sticks, to ascertain why loaded barges sailed faster than the current, or than unloaded barges; but his experiments only tended to confirm the fact that the heaviest end of a pole always went first with the current. The other was a plan for measuring a ship's way at sea, by means of a steel-yard and line, where a pound weight should indicate a mile, or more or less, according to the power of the instrument.

Feb. 23.—A letter from Mr. Knight to the President was read, containing some further observations on the sap of trees, the formation of radicles from the bark, and also that of the buds from the same source, instead of their being produced from the alburnum, as supposed.

A paper of Mr. Home's, on a peculiar joint discovered in the *squalus maximus* (basking shark), lately cast on the sea shore, was laid before the Society, accompanied by a drawing. More interesting particulars respecting the stomach of this fish are to form the subject of another communication.

XXX. *List of Patents for New Inventions.*

To Malcolm Mac Gregor, of Bell-yard, Carey Street, musical instrument maker, and William Mac Farland, of the Strand, umbrella manufacturer, for certain improvements in the construction of umbrellas and parasols. Dec. 29.

To John Brierley, of River Bank, in the county of Flint, for a process of setting blue lead, for corroding the same into white lead. Jan. 17, 1809.

To James Goddart, of Newman Street, gent., for his machinery for manufacturing a certain description of wooden boxes called chip boxes, or pill boxes, of all the various sizes and shapes hitherto made. Jan. 23.

To Edward Stracy, of Parliament Street, Westminster, esq., for an improved method of hanging the bodies, and of constructing the perches of four-wheel carriages, by which such carriages are rendered less liable to be overturned, and of constructing perch bolts and collar braces. Jan. 23.

To John Peek, of Charlotte-Row, Fort Place, Bermondsey, Surry, millwright, for a machine for casting printing types, by which three motions out of five made in the ordinary method of casting types are saved. Jan 23.

To Samuel Whitfield, of Church Street, Birmingham, brazier and scale-beam maker, for a method for the application of stamps, dies, and piercing tools, to the manufacturing of ears, handles and bewells for culinary articles of every description, whether in wood, iron, brass, copper, tin, silver, or any mixed metals. Jan. 23.

To Michael Logan, of Rotherhithe, civil engineer, for a transcendant ordnance, or improved cannon, for either marine, fort, or field service. Jan. 26.

To Anthony George Eckhardt, of Berwick Street, Soho, for a method of casting metallic and other bodies, together or separately, in moulds, in the state of fluidity or softness, in order that the said bodies may preserve the figures thus obtained, when they shall afterwards become solid, or consistent by cooling, or by any chemical or other change which shall or may take place, or be produced in the nature, order, proportions, or quantities of the component parts or ingredients of the same. Jan. 28.

Theatre of Anatomy, Greville-Street, Hatton-Garden.

Mr. TAUNTON's Lectures having been suspended through a severe indisposition, he begs leave to inform his pupils that he intends to resume them on Tuesday next, the 7th instant.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Carey, of the Strand,

For February 1809.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Thermometer. Noon.	Thermometer. 11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Jan. 27	45°	51°	48°	29·20	10	Stormy
28	49	54	47	·50	41	Fair
29	49	53	45	28·98	10	Stormy
30	48	48	44	·95	0	Stormy
31	37	47	40	29·85	32	Fair
Feb. 1	46	52	47	·62	21	Cloudy
2	51	51	48	·50	25	Cloudy
3	51	54	46	·20	24	Cloudy
4	46	49	42	·45	30	Fair
5	46	49	44	·35	15	Stormy
6	45	49	40	·40	18	Cloudy
7	39	39	34	·90	15	Cloudy
8	33	33	36	·79	8	Cloudy
9	40	51	47	·35	7	Cloudy
10	46	53	46	·16	8	Showery
11	46	52	47	28·75	0	Rain
12	46	51	45	·82	9	Showery
13	46	52	46	·97	51	Showery
14	47	51	46	29·30	30	Showery
15	46	52	44	·58	0	Rain
16	45	53	46	·58	39	Fair
17	46	54	47	·69	58	Fair
18	50	53	44	30·34	27	Cloudy
19	37	51	46	·40	36	Fair
20	46	52	47	29·98	37	Fair
21	39	42	36	30·05	47	Fair
22	33	43	41	·26	42	Fair
23	45	52	40	·12	45	Fair
24	42	47	41	·30	31	Fair

N. B. The Barometer's height is taken at one o'clock.

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XXXI. *Remarks on Hygrometry, and the Hygrometer of* J. Berzelius. *In a Letter from Mr.* J. Gough, *to Mr.* Tilloch.

Middleshaw, Feb. 25, 1809.

SIR,

Perhaps few additions could be made at present to the apparatus of a meteorologist, which would prove more acceptable than a cheap and correct hygrometer. Various attempts have been made to improve the instrument, which have commonly ended in adding to its sensibility: but such contrivances seem intended to amuse the superficial observer, rather than to assist the lover of meteorology. It is the business of those who cultivate this science, to determine the comparative humidity of the atmosphere, not only as it is found in different parts of the world, but also as it varies with situation in the same country. This project would be carried into effect with the greatest ease and certainty, by diminishing the sensible powers of the hygrometer rather than by improving them. The preceding proposal is far from being new; for the same notion respecting hygrometry occurred to Dr. Franklin, so long ago as the year 1764. The idea was suggested to the Doctor by an incident, which proves the atmosphere to be drier in Pennsylvania than it is in England and France. The Doctor's thoughts on the subject appeared in the first volume of the American Philosophical Transactions, under the form of a letter addressed to Mr. Nairne, of London, in 1780: and it is superfluous to add that his sentiments are delivered with the elegance and perspicuity which distinguish the productions of this great man.

I have entertained an opinion for some time past, that a common artist might construct an instrument which would answer the purposes already specified; and a few experiments were made under my direction three or four years ago, which were far from discouraging the hope. The plan of the hygrometer here alluded to is very simple, and will be easily understood from the following description, assisted by the annexed figure.

In Plate VI. Fig. 5, AD represents a scale made of brass, writing slate, or marble; in which the right line EF, nine or ten inches long, is divided into one hundred equal parts. This part of the instrument being prepared, take a square rod of wood, cut across the grain; it must be four or five inches in length, and not more than a quarter of an inch in diameter: this is represented in the plate by the rectangles PL, QK and MN. One surface of this rod must be marked longitudinally by a coloured line, which is denoted in the figure by EG and FH. After the rod has been thus marked, it must be suspended for several days in the air of a close vessel containing a quantity of quick-lime or dry potash, either of which will extract the moisture from the wood and bring it to a state of great dryness. When the piece is removed from the vessel, apply the end PE to the line PQ, making E coincide with the top of the divided line EF, and mark the place of G, the opposite extremity of the line EG. In the next place, suspend the rod for several days in the air of a close vessel containing a quantity of water, by which precaution the wood will become saturated with moisture. This being done, apply the end QF to the right line PQ, making F coincide with the bottom of the divided line EF, and mark the place of H the other extremity of FH. Lastly, draw the right line GH, which completes the hygrometer. As oft as you wish to make an experiment with this instrument, make the end PE of the rod PL coincide with the divided line EF, taking care that the point G falls in the line GH, and observe the degree marked by E, which denotes the state of the hygrometer. The figure MN shows how this is to be done; and it is placed in a situation which makes the point E coincide with 50°.

I have not the presumption to compare this apparently insignificant contrivance with the elegant hygrometer of M. De Luc. It has, however, the recommendation of simplicity; and a common artist might construct a number of them with little trouble and at little expense. They should all be cut from the same board, and made to the same scale; after which, those rods should be furnished with plates resembling the original one, and preserved for use, and which were found upon trial to give correspondent results in different parts of the scale. Dr. Franklin recommends slips of mahogany for hygrometrical purposes: my experiments, however, were made with rods of old dry deal, answering the preceding description. The preference was given to this kind of wood, because temperature has but little effect on its dimensions; besides which, I did not recollect at the time the recommendation given to mahogany by the celebrated philosopher of America.

Perhaps, sir, I should never have troubled you or any other journalist with the preceding trifle, had it not been for a paper which appears at the 39th page of your present volume. This is a description of a hygrometer recommended to the public, by the inventor J. Berzelius, on the supposition that Mr. Dalton's theory of mixed gases is consistent with the phænomena of meteorology. In attempting to vindicate the merits of his instrument, this foreigner asserts, that Dalton has proved the water of the atmosphere to be independent of the air. To this we may reply, It is a fact established by different philosophers of the last century, that water assumes the form of a gas under an exhausted receiver at any temperature greater than 32°. It has also been shown by M. De Luc, that the presence of air retards the production of the aqueous gas; but I will venture to say that M. Berzelius goes too far when he asserts that Mr. Dalton has proved the water of the atmosphere to be independent of the air. The perfect freedom of atmospherical vapour is unquestionably a favourite opinion with this gentleman; but in what part of his works, or in what manner, is the proposition demonstrated? An explicit answer to this question is absolutely necessary; because M. Berzelius has adopted principles in the construction of his hygrometer, which must remain precarious until the subject in debate has been decided in favour of the new hypothesis. M. Berzelius's remarks on Mr. Dalton's table, exhibiting the expansive force of vapour, have nothing to do with the business of hygrometry, so long as the preceding uncertainty can be objected to the opinions of the latter gentleman. This table may be true when confined to the phænomena of vapour produced in a vacuum; but I am convinced that it fails in point of accuracy when air is present, even if the experiment be made in Mr. Dalton's own manometer.

Every one who allows the justice of the preceding arguments must refuse his assent to M. Berzelius, when he says that the hygrometer should discover to what column of mercury the water gas of the air belongs; because we are far from being certain that the air contains any water gas at all.

The foregoing remarks relate to the hypothesis which suggested the hygrometer in question. As for the instrument itself, it cannot possibly have any just claim to correctness, before the following proposition is fairly established, in conjunction with the other new doctrines already mentioned. The substances of which these hygrometers have been or shall be made, exercise no attractive force on the aqueous gas; on the contrary, they only diminish the temperature of this gas; in consequence of which, part of it is condensed upon the cooling surface by the pressure of the rest. Hence it follows, that if this gas be equally cooled at the same instant by two different substances, it will fall in the form of water, and in equal quantities, upon them both. Various experiments, which I have made at different times, enable me to pronounce the proposition to be incorrect; consequently the instrument of M. Berzelius cannot be admitted into the apparatus of a meteorologist under the name of a hygrometer. To state this objection more clearly, let me be understood to say, that if any one would try two instruments of the kind at the same time, one of which consisted of glass and the other of tin or silver, they would assign different expansive forces to the water gas of the atmosphere, by beginning to condense it at different degrees of temperature. Besides, if we advert to some experiments by Count Rumford, it is evident that glass attracts water from the atmosphere when we suspect nothing of the kind, and that a glass globe is frequently covered with a thin film of dew when it is supposed to be dry. Moreover, the experiments of M. De Luc inform us that the phænomena of the hygrometer succeed in a vacuum, where it would be absurd to imagine

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gine the vapour to be condensed by a refrigerating principle residing in the whale-bone.—What are the genuine inferences from these facts? 1st, That glass and other substances rob the atmosphere of water by an attractive force; and that a loss of temperature does nothing more than prepare the air to part with its moisture more abundantly: 2dly, That the attraction of the hygrometer is resisted by a similar, but opposite power, residing in the atmosphere itself: 3dly, That the phænomena of the instrument are to be explained by the mutual reaction of these contrary forces.

There is one circumstance which renders the hygrometer of little or no use to the advocates of the new hypothesis: for, if I understand them rightly, a barometer will supply the place of the other instrument in their opinion. The author of this system maintains the joint pressure of the permanent gases to be 29·56 inches of mercury at all times and places, I suppose on the level of the sea. This being admitted, it will follow, that the same joint pressure may be found for any height; consequently, if this given quantity be subtracted from an observed elevation of the barometer, the remainder will express the expansive force of the aqueous atmosphere for the time and place. If this be what they mean, it must convince every meteorologist that their opinion cannot explain the changes that frequently take place in the weight of the atmosphere. For instance, if the hypothesis be true, the force of the aqueous atmosphere ought to be greatest in summer, and the barometer to be highest at the same season;—but this is not the case. On the contrary, the maximum of elevation commonly happens after the winter solstice, and the minimum too; for the barometer frequently rises during January to 30·60 or higher; *i. e.* the force of the atmospherical vapour is equivalent to one inch of mercury or more. The preceding conclusion cannot be reconciled both to observation and the hypothesis; for the thermometer very seldom reaches 48°, at least in Westmoreland, during the month of January; consequently the force of the aqueous atmosphere is never equal to $\frac{4}{10}$ths of an inch of mercury in this county at that season; and the hypothesis seems to confine the range of the barometer to the limits 29·56 and 29·96, which is inconsistent with observation.

I will conclude this long letter by the bare relation of the following experiment, which was made in a room where a fire is kept in winter.—February 21, 1809, De Luc's hygrometer stood at 52°, the thermometer at 50°, and the barometer 29·88 inches. It is proper to remark here, that the force of the permanent gases at Middleshaw cannot exceed 29·22 inches by the hypothesis, and must be considerably less by observation; consequently the pressure of the vapour was greater than ·66 of an inch. A silver vessel containing a quart or more had been placed near the thermometer at the commencement of the experiment, which became covered with a very thin film of dew when cooled down to 40°. I am, &c.,

JOHN GOUGH.

XXXII. *Hydraulic Investigations, subservient to an intended Croonian Lecture on the Motion of the Blood.* By THOS. YOUNG, M.D. *For. Sec. R.S.*

[Concluded from p. 133.]

III. *Of the Propagation of an Impulse through an elastic Tube.*

THE same reasoning that is employed for determining the velocity of an impulse, transmitted through an elastic solid or fluid body, is also applicable to the case of an incompressible fluid contained in an elastic pipe; the magnitude of the modulus being properly determined, according to the excess of pressure which any additional tension of the pipe is capable of producing; its height being such, as to produce a tension, which is to any small increase of tension produced by the approach of two sections of the fluid in the pipe, as their distance to its decrement: for in this case the forces concerned are precisely similar to those which are employed in the transmission of an impulse through a column of air enclosed in a tube, or through an elastic solid. If the nature of the pipe be such, that its elastic force varies as the excess of its circumference or diameter above the natural extent, which is nearly the usual constitution of elastic bodies, it may be shown that there is a certain finite height which will cause an infinite extension, and that the height of the modulus of elasticity, for each point, is equal to half its height above the base of this imaginary column; which may therefore be called with propriety the modular column of the pipe: consequently the velocity of an impulse will be at every point equal to half of that which is due to the height of the point above the base; and the velocity of an impulse ascending through the pipe being every where half as great as that of a body falling through the corresponding point in the modular column, the whole time of ascent will be precisely twice as great as that of the descent of the falling body; and in the same manner if the pipe be inclined, the motion of the impulse may be compared with that of a body descending or ascending freely along an inclined plane.

These propositions may be thus demonstrated: let *a* be the diameter of the pipe in its most natural state, and let this diameter be increased to *b* by the pressure of the column *c*, the tube being so constituted that the tension may vary as the force. Then the relative force of the column *c* is represented by *bc*, since its efficacy increases, according to the laws of hydrostatics, in the ratio of the diameter of the tube; and this force must be equal, in a state of equilibrium, to the tension arising from the change from *a* to *b*, that is, to $b-a$; consequently the height *c* varies as $\frac{b-a}{b}$, and if the tube be enlarged to any diameter *x*, the corresponding pressure required to distend it will be expressed by a height of the column equal to $\left(1-\frac{a}{x}\right).\frac{bc}{b-a}$, since $\frac{b-a}{b} : c :: \frac{x-a}{x} : \left(1-\frac{a}{x}\right)\frac{bc}{b-a}$. Now if the diameter be enlarged in such a degree that the length of a certain portion of its contents may be contracted in the ratio $1 : 1-r$, *r* being very small, then the enlargement will be in the ratio $1 : 1+\frac{r}{2}$, that is, x' will be $\frac{rx}{2}$; but the increment of the force, or of the height, is $\frac{ax'}{xx}.\frac{bc}{b-a}$, which will become $\frac{ar}{2x}.\frac{bc}{b-a}$. Now in a tube filled with an elastic fluid, the height being h, the force in similar circumstances would be rh, and if we make $h=\frac{a}{2x}.\frac{bc}{b-a}$, the velocity of the propagation of an impulse will be the same in both cases, and will be equal to the velocity of a body which has fallen through the height $\frac{1}{2}h$. Supposing x infinite, the height capable of producing the necessary pressure becomes $\frac{bc}{b-a}$, which may be called g, and for every other value of x this height is $\left(1-\frac{a}{x}\right)g$, or $g-\frac{ag}{x}$, or, since h becomes $\frac{ag}{2x}$, $g-2h$, so that h is always equal to half the difference between g and the actual height of the column above the given point, or to half the height of the point above the base of the column.

If two values of x, with their corresponding heights, are given, as b and x, corresponding to c and d, and it is required to find a; we have $\frac{b-a}{b}:c::\frac{x-a}{x}:d$, $dbx-dax=cbx-cba$, and $a=\frac{dbx-cbx}{dx-cb}$, or $\frac{b}{a}=\frac{dx-cb}{dx-cx}$. Thus if the height equivalent to the tension vary in the ratio of any power m of the diameter, so that, n being a small quantity, $x=b(1+n)$ and $d=c(1+mn)$, $\frac{b}{a}=\frac{bc((1+n).(1+mn)-1)}{bc((1+n).(1+mn-(1+n))}$ $=\frac{mn+n}{mn}$, since the square of n is evanescent, and $\frac{b}{a}=\frac{m+1}{m}$. For example, if $m=4$, $\frac{b}{a}=\frac{5}{4}$, and if $m=2$, $b:a::3:2$.

IV. *Of the Magnitude of a diverging Pulsation at different Points.*

The demonstrations of Euler, Lagrange, and Bernoulli, respecting the propagation of sound, have determined that the velocity of the actual motion of the individual particles of an elastic fluid, when an impulse is transmitted through a conical pipe, or diverges spherically from a centre, varies in the simple inverse ratio of the distance from the vertex or centre, or in the inverse subduplicate ratio of the number of particles affected, as might naturally be inferred from the general law of the preservation of the ascending force or impetus, in all cases of the communication of motion between elastic bodies, or the particles of fluids of any kind. There is also another way of considering the subject, by which a similar conclusion may be formed respecting waves diverging from, or converging to, a centre. Suppose a straight wave to be reflected backwards and forwards in succession, by two vertical surfaces, perpendicular to the direction of its motion; it is evident that in this and every other case of such reflections, the pressure against the opposite surfaces must be equal, otherwise the centre of inertia of the whole system of bodies concerned would be displaced by their mutual actions, which is contrary to the general laws of the properties of the centre of inertia. Now, if instead of one of the surfaces, we substitute two others, converging in a very acute angle, the wave will be elevated higher and higher as it approaches the angle: and if its height be supposed to be every where in the inverse subduplicate ratio of the distance of the converging surfaces, the magnitude of the pressure, reduced to the direction of the motion, will be precisely equal to that of the pressure on the single opposite surface, which will not happen if the elevation vary inversely in the simple ratio of the distance, or in that of any other power than its square root. This mode of considering the subject affords us therefore an additional reason for asserting, that in all transmissions of impulses through elastic bodies, or through gravitating fluids, the intensity of the impulse varies inversely in the subduplicate ratio of the extent of the parts affected at the same time; and the same reasoning may without doubt be applied to the case of an elastic tube.

There is, however, a very singular exception, in the case of waves crossing each other, to the general law of the preservation of ascending force, which appears to be almost sufficient to set aside the universal application of this law to the motions of fluids. It is confessedly demonstrable that each of two waves, crossing each other in any direction, will preserve its motion and its elevation with respect to the surface of the fluid affected by the other wave, in the same manner as if that surface were plane: and, when the waves cross each other nearly in the same direction, both the height and the actual velocity of the particles being doubled, it is obvious that the ascending force or impetus is also doubled, since the bulk of the matter concerned is only halved, while the square of the velocity is quadrupled; and supposing the double wave to be stopped by an obstacle, its magnitude, at the moment of the greatest elevation, will be twice as great as that of a single wave in similar circumstances, and the height, as well as the quantity of matter, will be doubled, so that either the actual or the potential height of the centre of gravity of the fluid seems to be essentially altered, whenever such an interference of waves takes place. This difficulty deserves the attentive consideration of those who shall attempt to investigate either the most refined parts of hydraulics, or the metaphysical principles of the laws of motion.

V. *Of the Effect of a Contraction, advancing through a Canal.*

If we suppose the end of a rectangular horizontal canal, partly filled with water, to advance with a given velocity, less than that with which a wave naturally moves on the surface of the water, it may be shown that a certain portion of the water will be carried forwards, with a surface nearly horizontal, and that the extent of this portion will be determined, very nearly, by the difference of the spaces described, in any given time, by a wave, moving on the surface thus elevated, and by the moveable end of the canal. The form of the anterior termination of this elevated portion, or wave, may vary, according to the degrees by which the motion may be supposed to have commenced; but whatever this form may be, it will cause an accelerative force, which is sufficient to impart successively to the portions of the fluid, along which it passes, a velocity equal to that of the moveable end, so that the elevated surface of the parts in motion may remain nearly horizontal: and this proposition will be the more accurately true, the smaller the velocity of the moveable end may be. For, calling this velocity v, the original depth a, the increased depth x, and the velocity of the anterior part of the wave y, we have, on the supposition that the extent of the wave is already become considerable, $x = \frac{ay}{y \mp v}$, taking the negative or positive sign according to the direction of the motion of the end; since the quantity of fluid, which before occupied a length expressed by y, now occupies the length $y \mp v$; and putting $a \backsim x = z$, $z = \frac{av}{y \mp v}$. The direction of the surface of the margin of the wave is indifferent to the calculation, and it is most convenient to suppose its inclination equal to half a right angle, so that the accelerating force, acting on any thin transverse vertical lamina, may be equal to its weight: then the velocity y must be such, that while the inclined margin of the wave passes by each lamina, the lamina may acquire the velocity v by a force equal to its own weight; consequently the time of its passage must be equal to that in which a body acquires the velocity v, in falling through a height b, corresponding to that velocity: and this time is expressed by $\frac{2b}{v}$; but the space described by the margin of the wave is not exactly z, because the lamina in question has moved horizontally during its acceleration, through a space which must be equal to b; the distance actually described will therefore be $z \pm b$, and we have $\frac{z \pm b}{y} = \frac{2b}{v}$, $z \pm b = \frac{2by}{v}$, $av \pm by - bv = \frac{2byy}{v} \mp 2by$, $y^2 \mp \frac{3}{2} vy = \frac{av^2}{2b} - \frac{v^2}{2}$, $(y \mp \frac{3}{4} v)^2 = \frac{av^2}{2b} + \frac{v^2}{16}$; but m being the proper coefficient, $v = m \surd b$, and $v^2 = m^2 b$, $\frac{av^2}{2b} + \frac{v^2}{16} = m^2 \left(\frac{a}{2} + \frac{b}{16}\right)$, $y = m \surd \left(\frac{a}{2} + \frac{b}{16}\right) \pm \frac{3}{4} v$, and $y \mp v = m \surd \left(\frac{a}{2} + \frac{b}{16}\right) \mp \frac{1}{4} v$. But when v is small, we may take $y \mp v$ nearly $m \surd \frac{a}{2}$, and $z = \frac{ma \surd b}{m \surd (\frac{1}{2} a)} = \surd (2ab)$, and $x = a \pm \surd (2ab)$, while the height of a fluid, in which the

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the velocity would be y, is nearly $a \pm \frac{3}{2} \sqrt{(2ab)}$: consequently, when the velocity v is at all considerable, y must be somewhat greater than the velocity of a wave moving on the surface of the elevated fluid; and probably the surface of the elevated portion will not in this case be perfectly horizontal; but where v is small, y may be taken, without material error, $m \sqrt{\frac{x}{2}}$, or even $m \sqrt{\frac{a}{2}}$, which is the velocity of every small wave. The coefficient m is here assumed the same for the motion of a wave, as for the discharge through an aperture, and I have reason from observation to think this estimation sufficiently correct.

Supposing now the moveable end of the canal to remain open at the lower part as far as the height c, then the excess of pressure, occasioned by the elevation before it, and the depression behind, will cause the fluid, immediately below the moveable plane, to flow backwards, with the velocity determined by the height, which is the difference between the levels; and the quantity thus flowing back, together with that which is contained in the moveable elevation, must be equal to the whole quantity displaced. But the depression, behind the moveable body, must vary according to the circumstances of the canal, whether it be supposed to end abruptly at the part from which the motion begins, or to be continued backwards without limit: in the first case, the elevation z will be to the depression as v to $y - v$, the length of the same portion of the fluid being varied inversely in that ratio; in the second case, the proportion will be as $y + v$ to $y - v$: and the difference of the levels will be $z + z \frac{y-v}{v} = \frac{zy}{v}$, or secondly $z + z \frac{y-v}{y+v} = \frac{2zy}{y+v}$; and first, $m \sqrt{\frac{zy}{v}}\, c + (y - v)\, z = (a - c)\, v$; but, since y is here considered as equal to $m \sqrt{\frac{a}{1}}$, putting $\sqrt{\frac{a}{2}} - \sqrt{b} = d$, $y - v = md$, and, calling $a - c$, e, $m \sqrt{\frac{zy}{v}}\, c + mdz = me \sqrt{b}$, $\sqrt{\frac{zy}{v}}\, c + dz = e \sqrt{b}$, $c^2 \frac{zy}{v} = e^2 b + d^2 z^2 - 2dze \sqrt{b}$,

$z^2 -$

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$z^2 - \left(\frac{c^2y}{a^2v} + \frac{2e\sqrt{b}}{d}\right)z = -\frac{e^2b}{d^2}$, and, calling $\frac{c^2y}{2d^2v} + \frac{e\sqrt{b}}{d}$, f, $z = f \pm \sqrt{\left(f^2 - \frac{e^2b}{d^2}\right)}$: and in the same manner f is found, for the second case, equal to $\frac{c^2y}{d^2(y+v)} + \frac{e\sqrt{b}}{d}$. For example, suppose the height a 2 feet, $b = \frac{1}{4}$, $c = 1$, and consequently $e = 1$, then d becomes $\frac{1}{2}$, $v = 4$, and $y = 8$; and in the first case $z = .1$, and in the second $z = .14$.

If v, the velocity of the obstacle, were great in comparison with $m\sqrt{\frac{a}{2}}$, the velocity of a wave, and the space c below the obstacle were small, the anterior part of the elevation would advance with a velocity considerably greater than the natural velocity of the wave: but if the space below the obstacle bore a considerable proportion to the whole height, the elevation z would be very small, since a moderate pressure would cause the fluid to flow back, with a sufficient velocity, to exhaust the greatest part of the accumulation, which would otherwise take place. Hence the elevation must always be less than that which is determined by the equation $m\sqrt{zc} = ev$, and z is at most equal to $\left(\frac{ev}{mc}\right)^2 = \frac{e^2}{c^2}b$; but since the velocity of the anterior margin of the wave can never materially exceed $m\sqrt{\frac{x}{2}}$, especially when z is small, and $\sqrt{\frac{x}{2}}$ being in this case nearly $\sqrt{\frac{a}{2}} + \frac{e^2}{2\sqrt{(\frac{1}{2}a)c^2}}b$, $m\sqrt{\frac{x}{2}} - m\sqrt{b} = m\left(\sqrt{\frac{a}{2}} + \frac{e^2b}{\sqrt{(2a)c^2}} - \sqrt{b}\right)$ which, multiplied by z, shows the utmost quantity of the fluid that can be supposed to be carried before the obstacle. Supposing $b = \frac{1}{2}a$, this quantity becomes $m\sqrt{\frac{a}{2}} . \frac{e^4}{c^4} . \frac{a}{4}$; and if $\frac{e}{c}$ be, for example, $\frac{1}{10}$, it will be expressed by $\frac{1}{40000}$ av, while the whole quantity of the fluid left behind.

A similar mode of reasoning may be applied to other cases of the propagation of impulses, in particular to that of a contraction moving along an elastic pipe. In this case, an increase of the diameter does not increase the velocity of the transmission of an impulse; and when the velocity of the contraction approaches to the natural velocity of an impulse, the quantity of fluid protruded must, if possible, be still smaller than in an open canal; that is, it must be absolutely inconsiderable, unless the contraction be very great in comparison with the diameter of the pipe, even if its extent be such as to occasion a friction which may materially impede the retrograde motion of the fluid. The application of this theory to the motion of the blood in the arteries is very obvious, and I shall enlarge more on the subject when I have the honour of laying before the Society the Croonian Lecture for the present year.

The resistance, opposed to the motion of a floating body, might in some cases be calculated in a similar manner: but the principal part of this resistance appears to be usually derived from a cause which is here neglected; that is, the force required to produce the ascending, descending, or lateral motions of the particles, which are turned aside to make way for the moving body; while in this calculation their direct and retrograde motions only are considered.

The same mode of considering the motion of a vertical lamina may also be employed for determining the velocity of a wave of finite magnitude. Let the depth of the fluid be a, and suppose the section of the wave to be an isosceles triangle, of which the height is b, and half the breadth c: then the force urging any thin vertical lamina in a horizontal direction will be to its weight as b to c; and the space d, through which it moves horizontally, while half the wave passes it, will be such that $(c-d).\ (a+\frac{1}{2}b)=ac$, when $c.e\ d=\frac{bc}{2a+b}$. But the final velocity in this space is the same as is due to a height equal to the space, reduced in the ratio of the force to the weight, that is, to the height $\frac{bb}{2a+b}$, and half this velocity is $\frac{1}{2}m\surd\left(\frac{bb}{2a+b}\right)$, which is the mean velocity of the lamina. In the mean time the wave describes this space $c + d$, and its velocity is greater than that of the lamina in the ratio of $\frac{c}{d} + 1$ to 1, that is $\frac{2a + b}{b} + 1$ or $\frac{2a}{b} + 2$ to 1, becoming $m\left(\frac{a}{b} + 1\right)\frac{b}{\sqrt{(2a + b)}} = m \frac{a + b}{\sqrt{(2a + b)}}$; which, when b vanishes, becomes $m\sqrt{\frac{a}{2}}$, as in Lagrange's theorem, and, when b is small, $m\left(\sqrt{\frac{a}{2}} + \frac{3}{4}\frac{b}{\sqrt{(2a + b)}}\right)$, or $m\frac{a + \frac{3}{4}b}{\sqrt{(2a)}}$; but if a were small, it would approach to $m\sqrt{b}$, the velocity due to the whole height of the wave.

XXXIII. *On the Icy Crust formed on Glass Windows during a severe Frost:—with a few Remarks on Marine Vegetables. By Mr.* JAMES GRAHAM, *of Berwick-upon-Tweed.*

TO MR. TILLOCH,—SIR,

IF you think the following observations on the crust formed on windows during a severe frost merit a place in your very useful and entertaining Miscellany, they are much at your service.

This curious phænomenon is so common, that I believe there are very few who have not taken some notice of it; but, like many of the other appearances in nature, which strike the mind of the philosopher or the contemplative observer with wonder and astonishment, with the great bulk of mankind it excites not the least surprise. Such seems to be the general weakness of the human intellect, that we all require some friendly hand or kind assistant to first "rear the tender thought, or teach the young idea how to shoot." The appearance to which I wish to call the attention of your readers is the various figures which are represented on the glass where this crust is formed. I have found some, whose curiosity was in a certain degree excited, suppose that all was merely accidental, or formed by what we often call chance: but, on a closer observation, this will not be found to be the case;—when strictly examined, every figure is as regularly formed as if drawn by the hand of a skilful artist, and the whole exhibits, as it were, a beautiful delineation of various marine or sea plants. Sometimes there is an exact representation of the plant from which that species of ashes or alkali commonly called kelp is made;—on other parts of the glass will be seen a perfect likeness of some of the smaller vegetable productions, which, from a small root, branche out into an astonishing number of very fine fibres, joined together in such curious workmanship as far to excell any land production (at least that I have observed); indeed no description which I can give, without a drawing, can convey any idea either of the beauty or curiosity of these several icy arborifications. A few of the larger kinds I have sometimes observed during a continued frost; but the more common appearances resemble the plant from which the kelp is made, and the smaller vegetable productions. I wish to be informed by any of your learned and philosophical readers, What can be the natural cause which produces this effect? Surely we cannot ascribe it to mere accident; for if this were the case, there certainly would not be the same regular uniformity. It may, however, be necessary to observe, that this uniform appearance will sometimes be broken; but on strict examination I have always found it to proceed from some such circumstance as a sudden change in the temperature of the air in the room by an increase of company, or a larger fire, &c.: these will sometimes in a certain degree melt the crust on the glass, and if again suddenly frozen the regularity of the figures will appear broken; but where Nature is left to operate without interruption, I have always found the result the same.

I can scarcely omit this opportunity of directing your attention to another observation, which I doubt not will be grateful to some of your readers, as enlarging the sphere of their curiosity, and giving a more ample scope to the contemplative mind:—

The first sight of the sea to a person who has lived to the years of maturity without seeing it, (and even in this island there are many,) I am apt to think is the greatest object which Nature presents to the mind on this terraqueous globe. If the survey is made on a calm summer's day, the clear, smooth, and extended surface, which is bounded only by the horizon, stretching as far as the eye can reach, fills the mind with the most pleasing wonder and surprise; while the imagination is left to roam at large on the supposed expanse which lies far beyond the circle which the eye can embrace. If the survey is made in the winter, especially during a storm, what a grand and awful spectacle is presented, particularly to any person not accustomed to the scene! The deep and hollow sound of contending waves catches the ear at a great distance: but when the eye comes to survey a seemingly boundless ocean rolling in constant succession its tremendous billows, till they dash with such impetuous force on the shore as to threaten destruction to the very rocks and banks which Nature has placed as a barrier to its almost irresistible fury, the mind is filled with amazement.

These are appearances so peculiarly grand, that they arrest the attention of even the careless and indifferent spectator. The objects to which I beg leave to direct the inquisitive mind, which finds pleasure in observing and examining the great variety of Nature's productions, is the vast quantity of marine plants which are to be found on the shore and amongst the rocks during the ebb tide:—these are often thrown up in such promiscuous heaps on the beach, that superficial observers do not think they can deserve any attention; nor will they easily be induced to believe that the mind of the traveller is not more astonished when he first visits the torrid zone, and finds every tree, every shrub and plant, in short the whole vegetable creation, different from any thing he had seen in the more temperate climates, than any person will be when he first examines the productions of the ocean. I shall not attempt to give any delineation of this vegetable kingdom: even if my abilities were equal to the task, it would far exceed the limits of a short essay: but I will again assure all who have not made the experiment, that their pains and trouble will be amply repaid, and their curiosity fully gratified.

fied. I cannot, however, conclude without one more observation:—During the ebb tide, if the shore is rocky, a number of small pools of clear water are left, in which will be found many of the smaller plants adhering to the stones or rock, which, if carefully removed, and, before they are too dry, spread on white paper, will exhibit a most beautiful and pleasing variety without the trouble of drawing.

I am, sir, your most humble servant,

JAMES GRAHAM.

Berwick-upon-Tweed,
Feb 1, 1809.

XXXIV. *A Letter on the Alterations that have taken place in the Structure of Rocks, on the Surface of the basaltic Country in the Counties of Derry and Antrim. Addressed to* HUMPHRY DAVY, *Esq., Sec. R. S. By* WILLIAM RICHARDSON, *D. D.*

[Concluded from p. 116.]

Inquiry into the Formation of our perpendicular Façades.

It is natural that the first great operation we proceed to investigate, should be the formation of our magnificent façades, one of which is the principal subject of this memoir.

The line of coast that bounds our basaltic area on its north side; extends about twenty-five Irish miles, in which course the precipices are nearly continuous, and more than one half of them absolutely perpendicular for a great part of their stupendous height. The operation by which they were cut off so abruptly, and left with a formidable aspect towering over our coast, is the one we inquire into.

That these bold precipices once projected further in many places is easily demonstrated; at *Beanyn Daana*, and at the *Chimney*, the columnar construction was obviously once carried much further out.

At the *Milestone*, *Portcooan*, and *Portnabau*, the fragments of dykes extend far beyond the face of the precipice.

These same facts, together with the projecting base, show that these sudden abruptions were not formed by the subsiding,

siding, and sinking of one part, leaving the remainder in its place; still less by any violent revolution, or convulsion, as the stratification has not sustained the slightest shock either above or below the façade.

The formation of our abrupt coast, has been ascribed to the action of the sea beating violently against it, washing away the lower parts, and leaving a perpendicular façade standing; as we often see on the banks of rapid and encroaching rivers.

A cool examination of our precipices will soon prove that our façades could not have been so formed; for we always find them on the highest part of the cliff, and receding from the water, which could be instrumental in bringing down the materials from above, only by washing, and so wearing away the bases of the steepest parts; but the elevations of these bases are utterly irreconcileable to this supposition; for instance, the base of *Pleskin* façade is two hundred feet above the present level of the sea, that of *Fairhead* three hundred: now had the sea ever risen to either height, it would have submerged a great part of *Ireland*, and none of the neighbouring country (whatever its level may be) bears the least resemblance to alluvial ground, nor shows any mark of having been once covered by the sea.

The next argument is still more conclusive: the boundary of our basaltic area on its north side, is for twenty-five miles also the confine of sea and land; so far it is natural to ascribe its features, and characteristic marks, to the action of the powerful element that beats against it. But when that precipitous boundary ceases to be the confine of sea and land, turns southward towards the interior, and becomes the line of demarcation between the basaltic and schistose country on the west, it still preserves its former character: that is, of a range or ridge of very high land, steep to the exterior, and sometimes cut down vertically into façades, like its northern part that lines the shore.

Thus *Magilligan Rock* (four miles inland) is not inferior in magnificence to any of our façades on the coast: its perpendicular section is one hundred and seventy feet, and this continuous for a mile; the façades at *Bienbraddock* are nine miles further inland, and those of *Monyneeny* thirteen; while the base of the lowest of these perpendicular precipices is elevated 1400 feet above the sea.

The same style prevails on the east side of our basaltic area, after its boundary ceases to be the confine of sea and land; for the limestone façades at *Garron Point* (considerably above the level of the sea) exactly resemble those of *Dunluce* and *Kenbaan* at the water edge; and *Cave Hill* (several miles from the sea, and nearly one from the shallow estuary of *Belfast*,) exhibits basaltic façades at the height of one thousand feet, precisely similar, and little inferior to those of *Magilligan*.

The exact resemblance between our inland façades (on the east and west sides of our area) to those on the shore, proves them to be all effects from the same cause, and that our accumulated strata have in all these similar instances been cut down vertically by the same agent, and that this agent was not the sea.

Nor has this powerful agent confined its operations to our coast, or to the periphery of our basaltic area; we can trace it over its whole surface; we find throughout its interior, similar, though very diminutive abruptions, executed precisely in the same manner, that is, strata cut across by a long vertical façade, their planes on the upper side perfectly undisturbed, while on the lower side all the materials of which that part of the stratum was once composed are completely carried off.—(See 6th fact.)

We are now unavoidably led into a discussion of a question which has at all times occupied the attention of naturalists.

Whence arise the Inequalities with which the Surface of the Earth is so exceedingly diversified?

I shall not attempt to encounter this question generally, nor to extend my inquiries beyond the limits I have prescribed to myself; but I shall try whether the curious facts so profusely exhibited over our basaltic area, throw any light upon the formation of our own inequalities, or lead us a step

towards

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towards the discovery of the operations by which such stupendous effects have been produced.

Some, to escape the difficulties in which this question is involved, ascribe our inequalities to original formation; as if the world had come from the hand of the Creator with the variegated surface which now contributes so much to its beauty; but the frequent interruptions, and resumptions of the strata in our area, with the perfect resemblance of the corresponding parts, however great the interval by which they are separated, can scarcely leave a doubt that these strata were at first continuous; of course, the figure of our surface at that time must have depended on the original positions and inclinations of these strata, which, as appears by the 3d fact, are now unconnected with the superficial line, the figure of which is governed by their abruptions and removals alone.

Naturalists have differed much in opinion as to the direction in which the causes acted that produced the inequalities on the surface of our globe; some referring us to the bowels of the earth as to the scene of action; while others assert that the operations were performed upon the surface itself.

But the slightest inspection of our façades will at once prove that the first hypothesis cannot be correct; for obliquity of direction must have been the result of a disturbing cause acting from below; whereas parallelism and a steady rectilineal course distinguish the basaltic arrangements of which I have been treating.

We have, it is true, occasional depressions of our strata, where they obviously have subsided, and no doubt from a failure of support below; but in no instance that I have met with, in our area, are these attended by the slightest concussion; the permanent and subsided parts, with us still preserve their parallelism, and the continuity of their material; whence it is probable this event took place previous to the induration of the strata, and of course antecedent to the period to which I limit myself.

Buffon ascribes our superficial inequalities to the agitation of the waters while they covered our earth, and argues from the resemblance these inequalities bear to the waves of the sea; a resemblance I cannot trace in any country which I have observed, nor could our sudden and perpendicular abruptions ever have been produced by any agitation of the waters.

Professor Playfair considers rivers as having formed not only the beds or channels in which they flow, but also the whole of the valleys through which they run, and in general all the inequalities of our surface; but an attentive observer, tracing the course of any of our most rapid rivers, would soon perceive that the quantity of its depredations have been comparatively insignificant, and that they can be determined with precision: the river has no doubt in several places extended itself considerably on both sides, but in the intermediate space between the remotest boundaries it ever reached, it levels, instead of raising inequalities.

The same result I apprehend would follow from the operations of another agent, which theorists are in the habit of calling in to their aid, when they cannot find some certain material, which from their theory we had reason to expect; they then tell us it has been carried off, and lost in the suite of degradations and decompositions.

But decay and decomposition, instead of creating inequalities, would produce a contrary effect, and deface those actually existing; they would gradually abate the height of our perpendicular façades, and increase the green steep at their bases by the accumulation of the crumbling and mouldering materials from above; while the more diminutive façades formed by the abruptions of single strata scattered over the face of our area, and forming its most characteristic feature, would in time (as many are already) be converted into steep acclivities covered with verdure.

Such are the principal causes to which the inequalities of our surface have been generally ascribed. Previous to our deciding finally upon their insufficiency, it may be proper to enumerate a few of those inequalities, where the deviation of our present surface, from the form it probably had originally, is not only striking, but where also the concomitant circumstances afford demonstration, that some great operation has once taken place there.

Thus, by making ourselves acquainted with effects, we shall be better qualified to investigate causes; and if those effects shall appear to be beyond the powers of such natural agents as we are already acquainted with, we shall be justified in admitting the performance of operations to which we have seen nothing similar; and also in admitting the former existence of powers of far superior energy to any we have ever known in action.

Enumeration of some remarkable Inequalities in the Surface of our basaltic Area, produced since the Consolidation of its Strata.

That we may better understand the facts I am proceeding to state, I shall assume (in the style of the mathematicians *puta factum*) previous to demonstration, that the planes of our uniform, rectilineal strata, however interrupted we may now find them, were once continuous.

Upon this supposition, the valley of the *Mayola*, between the stratified summits of *Seafin* and *Slievegallon*, is an excavation 1700 feet deep, and three miles wide, of which the whole materials have been completely carried off.

To the northward of this excavation, between *Seafin* and *Carntogher*, the continuous accumulated strata of basalt are interrupted, and taken away quite down to the schistose substratum; while the untouched summits of the contiguous mountains, *Carntogher*, *Seafin*, and *Monyneeny*, are still stratified basalt.

On the eastern side of our area, immediately to the southward of *Kello* and *Connor*, a similar operation has been performed, attended by still more extraordinary circumstances.

We here find a district near four miles in diameter, called the *Sandy Braes*; over this whole space the basaltic stratification has been carried off, and the operation has reached deep into a very singular substratum, a reddish breccia, by some mineralogists called a porphyry, the mass friable, but the component angular particles of extreme hardness.

The hills, of which this little district is full, are every one perfect segments of spheres, while the loftier basaltic hills that surround it preserve their characteristic form, to wit, a gradual acclivity on one side, with a steep abruption on the other.

As we sail along our northern shore we discover another great chasm or interruption of our strata, which also cuts deep into the substrata: for on the west side of *Ballycastle* pier, the bold basaltic precipices suddenly disappear, and at a lower level disclose the substratum, which appears to be an alternation of sandstone, and coal, sometimes with bituminous schistus.

A mile or two to the eastward the abrupt precipice is resumed, and a basaltic stratum again occupies its summit on to *Fairhead,* with the same angle of inclination in which it was disposed along our whole coast, that is, a slight ascent to the north.

Traces of similar operations and abruptions are to be found over our whole area, but the preceding are sufficient to make us acquainted with the style of these interruptions of our strata; of course it is time to proceed to the suspended demonstration, that our strata, so interrupted, were once continuous, notwithstanding the magnitude of the interval by which the corresponding parts are now separated.

Proofs that our now interrupted Strata were once continuous.

We must now turn back to the façades of *Bengore,* where the strata themselves, and all the circumstances attending them, are so happily displayed, as to throw great light on the subject, and to lead us analogically, step by step, to the conclusion we seek for.

Let us examine and trace the summit of the precipice for a mile immediately eastward from the *Giant's Causeway,* and we shall find a frequent interruption and resumption of the fourth, fifth, and sixth strata, at the shortest intervals, the interruption not always reaching to the lowest of the three, which in that case remains continuous; so far simple inspection removes all doubt, that each of these strata was once continuous as far as the great depression to the west of *Pleskin.*

Here indeed the interruption becomes considerable, not less

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less than a mile; but when we find at *Portmoon* a succession of three strata with the same inclination, in the same order, of the same thickness each, and with the same strong characteristic marks that distinguished the three interrupted, at the depression; above all, when we find the strata they rest upon continuous (at least with very trifling interruptions) for the same extent; I think we can scarcely entertain a doubt that this interval between the corresponding parts, though so much greater than any of the preceding, is, like them, but an interruption, and that these strata were once continuous from the depression to *Portmoon.*

The same style of induction would establish the quondam continuity of all the strata in the face of *Bengore* promontory, for here the strata are so distinctly marked that we know each of them when we find it again after any interruption.

In the rest of our precipices and façades, the similarity of the strata deprives us of this advantage; yet in their smaller interruptions, the eye, by tracing the rectilineal course of the strata, and so connecting the separated parts, can establish their former continuity: while in the greater intervals we must rest our proof on analogy alone.

That we may be entitled to carry this style of induction into the interior of our basaltic area, and to apply the same reasoning to enable us to form a similar decision upon the more stupendous interruptions of our strata, which I have already enumerated, it becomes necessary to explain the geological construction of our area,—the strata of which it is formed—their arrangement—and their inclinations.

An extensive limestone stratum, white as chalk, and about two hundred feet thick, seems to form the base of the whole district I limit myself to: upon this, accumulations of rectilineal and parallel basaltic strata, are heaped up to most unequal heights.

This great calcareous stratum seems not to be an accurate plane, but rather to resemble a bason, as every where at its periphery it dips to the interior; yet the plane of its section has a slight ascent to the southward; a recollection of these circumstances will enable us to account for every appearance this stratum exhibits, as it happens to be disclosed to us; and by the converse, an attention to these appearances will enable us accurately to determine the position of the stratum.

This stratum, from *Ballycastle* to *Solomon's Porch*, (about twenty-five miles,) keeps very nearly the level of the sea, often indeed sinking below the surface, but never raising its lower edge above it: but when at *Solomon's Porch*, the boundary of our basaltic area begins to deflect to the south-west, and then to the south, the ascent of the stratum to the southward begins to operate, and we perceive the dotted line of its quarries gradually to rise along the face of the mountain from the shore to *Monyneeny* and *Seafin*, where is has attained the height of 1500 feet: it is true, the actual stratum has not been opened at these two great elevations, but the white rubble immediately below the basaltic façade proves incontestably that it is close at hand.

An accumulation of basaltic strata, had in this southern course, as well as on the northern shore, covered the limestone up to the summits of the hills or mountains.

I have already stated how the ridge of mountain is suddenly interrupted by the valley of the *Mayola*, from 1600 to 1700 feet deep; but if we look to the southward, in the rectilineal course of the strata (the positions of which we have been able to ascertain with so much accuracy), we shall find near the summit of the mountain *Slievegallon* a similar white limestone stratum crowned with basalt, cutting it in the very direction the former ought to have reached it, that is perhaps two hundred feet higher, the ascent of the strata to the southward having elevated their planes so much in a distance of four miles, the probable interval between the summits of these mountains.

We are now to decide whether this calcareous and basaltic fragment, on the summit of *Slievegallon* mountain, be the last remnant of the old arrangement we have been tracing, and ascertaining with so much precision, for seventeen or eighteen miles from the sea, and twenty-five miles along the coast, but now interrupted by the valley of the *Mayola*, like our former more diminutive interruptions, and also like them resumed at the next elevation, in the same rectilineal course, the strata preserving the same order, and the same characteristic marks. Or whether these strata, appearing on the summit of *Slievegallon*, be the commencement of a new arrangement, abandoned by nature as soon as begun: which is highly improbable, for neither limestone nor basalt are to be found on the mountain except in this solitary hummock.

We might, by a minute attention to the inclinations and arrangements of the strata contiguous to the other interruptions I have enumerated, prove in like manner that the basaltic masses crowning the summits of the surrounding hills and mountains, are merely the remnants of strata once extensive and continuous, but interrupted and carried off, as in the preceding case, by the same powerful agent.

The more diminutive inequalities scattered over the whole surface of our area, and always produced by interruptions of the strata, would still more easily admit the application of the same reasoning, from the contiguity of their abrupted parts; but the detail would be tedious; those who wish to pursue the subject further must come to the scene themselves.

Materials completely carried off.

A circumstance perhaps still more extraordinary, is the complete removal of all the materials that once filled up the intervals between the abrupted parts of these strata. I have stated in my 9th fact, that the materials that had formerly composed the projecting parts of our northern façades and precipices, had totally disappeared.

The removed parts of the limestone stratum on the west side of our area have shared the same fate, for where the chain of mountains extending from *Magilligan Rock* to *Bienbraddock*, is interrupted by valleys at *Stradreagh*, *Drumrommer*, and *Ballyness*, it is obvious that the limestone stratum was once continuous to the high points where it shows itself on *Keady*, and the mountains on each side; its thickness too, wherever we can try it, is very great; yet this stratum, which in its entire state must have spread like a roof far above the present surface of these valleys (which are now sunk deep into the schistose substratum) has not left a particle of its *debris* behind, nor is a single lump of white limestone to be found, until we come to the quarries, that is, to the edge of the solid, untouched stratum.

Conclusions.

The conclusions that unavoidably follow, from the consideration of these facts, are,

That the hills and mountains, in the district I have been describing, were not raised up or formed as they now stand, but that they are the undisturbed remains of strata that were left behind, when stupendous operations carried away the parts that were once contiguous to them.

That the inequalities of this surface were all produced by causes acting from above, and carrying off whatever they touched, without in the least disturbing what was left behind.

Additional Evidences. Basaltic Hummocks.*

The arguments on which I have founded my opinions have hitherto been all taken from the hollows in our surface, and the interruptions in our strata, both which, the concomitant circumstances have led me to consider as so many excavations; but the lofty elevations, and the abrupt prominencies rising suddenly from our surface, when minutely examined, lead us irresistibly to the very same conclusion.

When you and I examined together the line of our northern façades, we studiously sought for the points where nature had made any change in her materials or their arrangement, hoping that at the junctions of these little systems, we should find some facts that would throw light on the subject; but we generally failed; want of perpendicularity, or other local circumstances, impeding us at the most interesting points.

On the present occasion she has adopted an opposite line of conduct, and in many of the steps she has taken, obtrudes upon us demonstration of what she has done.

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* Navigators use the word hummock to express circular and elevated mounts, appearing at a distance; I adopt the term from them.

Whoever

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Whoever has attended to the exertions of man when employed in altering our present surface, either by levelling heights, or by making excavations, must nave observed that it is the practice of the workmen to leave small cylindrical portions standing, for the purpose of determining the height of the old surface, and thereby ascertaining the quantity of materials removed.

To these may be compared the stratified basaltic hummocks so profusely scattered over our area, and which seem to show how high our quondam surface once reached.

The hummock of *Dunmull*, three miles south-east from *Portrush*, is very beautiful, it stands on the top of a high ridge, and is a conspicuous object from all parts of the country; it is exactly circular, its flat surface contains an acre, it is completely surrounded by a perpendicular façade about twenty-five feet high, and formed by two strata, a columnar, and an irregular prismatic, resting upon it.

From this elevated station, where I had the pleasure of accompanying you, I showed you at six or seven miles distance to the westward, among the *Derry* mountains, the still loftier hummocks of *Altabrian* and *Sconce*, hemispherical in form, composed of but one stratum each, while their swelling out bases displayed accumulations of many more: and also near those the hummock of *Fermayle*, resembling *Dunmull*, but much larger, and also surrounded by a façade composed of two strata.

I showed you also at twenty miles distance to the south-east, the gigantic *Slemish*, one of our basaltic hummocks, magnified (as it were) into a lofty and insulated mountain, completely stratified from its base to its flat summit.

I showed you likewise from the bottom of its ridge, the neat but diminutive hummock, called the *Rock of Clogher*, above *Bushmills*. As our time was precious, you took my word for its stratification being precisely similar to that of *Dunmull*.

There are many other basaltic hummocks scattered over the surface of our area, all of them either stratified or portions of strata; two of the most [illegible] hill of

ock

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Knock Loughran, near *Magheta*, and a tall hummock (whose name I forget) a mile eastward from *Lisanoure*.

We meet still more frequently an imperfect style of hummock, a semicircular façade one side, while on the other it slopes away gradually with the dip of the strata, as if the operation had been interrupted before it was carried quite round; the most remarkable of these are *Ballystrone*, in *Derry*, and *Croaghmore*, in *Antrim*, both visible from *Dunmull*.

Regular stratifications on the summits of hills and mountains have been long a stumbling-block to theorists; the historian of the French Academy, for the year 1716, obviously ascribing the superficial inequalities of the earth, (like many others) to causes acting from below, and perceiving how incompatible such assemblages of strata were to his theory, thinks it safer to doubt their existence, as they could not have been formed, he says, "unless the masses of the mountains were elevated in the direction of an axis perpendicular to the horizon: *ce que n'a pu être que très rare.*"

But as these stratified mounts are in our area by no means uncommon, they lay us under the necessity of suggesting another alternative similar to those we have already stated.

Were these isolated hummocks originally formed as they now stand, (solitary and separate from each other) one by one; or, are they the last remaining portions of a vast consolidated mass, of which the intermediate and connecting strata have been carried off by causes with which we are unacquainted?

To be able satisfactorily to resolve this alternative, it becomes necessary to take a careful view of the contiguous countries, and to try whether their construction, and the arrangement of their strata, will throw any light upon the subject.

When we examine the assemblage of hummocks above *Knockmult*, that is, *Sconce*, *Fermoyle*, and *Altabrian*, we find their materials and stratification precisely similar to that of the country below them to the eastward, where the abruptions of the strata are displayed in long stony ridges—to the south,

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south, the abruptions on the summit of *Keady* mountain discover the same similarity; and to the north-west the grand façade of *Magilligan Rock*, three miles distant, displays an accumulation of exactly the same sort of strata consolidated into an enormous mass.

The hummock of *Dunmull* is formed of two very particular strata, a columnar, and an irregular prismatic; but I showed you, a mile to the northward, at the façades and quarries of *Islamore* and *Craigahuller*, at the base of the hill, that the whole ridge, on the summit of which *Dunmull* is placed, was a consolidated mass, formed by alternate strata of the same description: and that the arrangement of the whole country below, and adjacent, was precisely the same with that of the hummock of *Clogher*, I proved to you at the curious opening of the strata at *Bushmills Bridge*, and in the façades at the *Giant's Causeway*.

After these proofs that so many (and I might proceed to the rest) of our detached hummocks are in their construction and materials, similar to, and connected with, the main consolidated masses of which our country is formed, I think it will scarcely be asserted that these hummocks were originally formed, solitary and separate as they now stand; but rather that they were once parts of that vast whole, and left behind in their present form, upon the removal of the contiguous portions of their strata, by some powerful agent, of whose operations and modes of acting, we have hitherto obtained little knowledge.

The highest point on the façade of *Cave Hill* is called *M'Art's Castle*, and appears to be a solitary fragment of a stratum, precisely similar to those below it, and obviously once extended like them.

The irregularity of the summit of *Fairhead*, plainly shows that its gigantic columns once reached higher.

And in the façade of *Magilligan*, the highest of all, a few desultory patches of an upper stratum (no doubt once perfect and continuous) are to be traced along its summit.

Our mountains themselves seem to show clearly that they were once higher; the top of *Magilligan* mountain is an extensive plain, over which a wandering stratum is interrupted and resumed at intervals for a great way.

At the highest part of *Donald's Hill,* over the valley of *Glenmuller,* we find a hummock; also at the termination of the ridge, at its highest part over the valley of *Mayola,* similar hummocks. I have the honour to be, sir,

your obedient humble servant,

W. RICHARDSON.

Clonfecle, Jan. 2, 1808.

XXXV. *Method of Preserving Fruit without Sugar, for House Use or Sea Stores. By Mr.* THOMAS SADDINGTON, *of Lower Thames Street, London**.

SIR,

I SHALL be much obliged to you to lay before the Society of Arts, &c., the inclosed communication, and a box containing the following fruits in bottles, preserved without sugar; namely, apricots, gooseberries, currants, raspberries, cherries, Orleans plums, egg plums, green gages, damsons, and Siberian crabs. I have also sent some fresh English rhubarb plant, preserved in a similar manner. The same mode is applicable to other English fruits, as cranberries, barberries, and many more. This manner of preserving fruit will be found particularly useful on ship-board for sea stores, as the fruit is not likely to be injured by the motion of the ship, when the bottles are laid down on their sides, and the corks kept moist by the liquor, but on the contrary will keep well even in hot climates.

The cheapness of the process will render it deserving of the attention of all families from the highest to the lowest ranks of society. If the instructions I have sent are well attended to, I have no doubt that whoever tries my method will find it to answer his expectation.

I am, sir, your most obedient humble servant,

THOMAS SADDINGTON.

London, Jan. 8, 1808.

To C. TAYLOR, M.D. Sec.

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* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1807.——Five guineas were voted by the Society to Mr. Saddington for this communication.

A new

A new Method to preserve various Sorts of English Garden and Orchard Fruit, without Sugar.

GENTLEMEN,

THE general utility, as well as luxurious benefit, arising from the fruit produced by our gardens and orchards, is well known and acknowledged at the festive board of every family; nor is this utility and benefit less manifested by a desire of many persons to preserve them for culinary purposes, in the more unbountiful season of the year; and I am well persuaded that this commendable desire would be greatly extended in most families, was it not attended with so much expense as is generally the case by preserving fruit in the common mode with sugar, that article chiefly constituting the basis by which it is effected. In addition to the expense of sugar, which is frequently urged as a reason for not preserving, there are other objections to that method, and what I am about to mention cannot be considered as the least, namely, the great uncertainty of success, occasioned by the strong fermentable qualities contained in many sorts of fruit. It may be said by some, that fruit may be preserved for a length of time without sugar by the ordinary mode of baking or boiling, and being closely stopped up, to which assertion I freely assent; but even that method is frequently attended with uncertainty; for if the cork or other means used for keeping the external air out of the vessel becomes dry, or from any other cause the atmospheric air exchanges place with what is impregnated by the fruit, it soon becomes mouldy and unfit for use.

From these considerations, and a desire of preserving fruit at a trifling expense, I have made various and successful experiments of doing it without sugar, and at the same time with a certainty of their retaining all those agreeable flavours which they naturally possess; and it is highly probable that they will keep perfectly good for two or three years, or even a longer period, in any hot climate, by which it appears to become a valuable store for shipping or exportation, as I have exposed them to the action of the meridian sun in an upper room, during the whole of the summer, after they have been so preserved (being done in 1806). I have now the pleasure of laying before the Society specimens of the fruit alluded to.

Process for preserving Fruit.

The bottles I chiefly use for small fruit, such as gooseberries, currants, cherries, and raspberries, are selected from the widest-necked of those used for wine or porter, as they are procured at a much cheaper rate than what are generally called gooseberry bottles. Having got them properly cleaned, and the fruit ready picked, (which should not be too ripe,) fill such of them as you intend doing at one time, as full as they will hold, so as to admit the cork going in, frequently shaking the fruit down whilst filling. When done, fit the corks to each bottle, and stick them lightly in, so as to be easily taken out when the fruit is sufficiently scalded, which may be done either in a copper, or large kettle, or saucepan over the fire, first putting a coarse cloth of any sort at the bottom to prevent the heat of the fire from cracking the bottles: then fill the copper, or kettle, with cold water sufficiently high for the bottles to be nearly up to the top in it: put them in side-ways to expel the air contained in the cavity under the bottom of the bottle; then light the fire if the copper is used, taking care that the bottles do not touch the bottom or sides, which will endanger their bursting; and increase the heat gradually until it comes to about one hundred and sixty or one hundred and seventy degrees, by a brewing thermometer*, which generally requires about three quarters of an hour. For want of such an instrument it may be very well managed by judging of the degree of heat by the finger, which may be known by the water feeling very hot, but not so as to scald it. If the water should be too hot, a little cold may be added to keep it of a proper temperature, or the fire may be slackened. When it arrives at a sufficient degree of heat, it must be kept at the same for about half an hour longer, which will at all times be quite enough, as a longer time, or greater heat, will crack the fruit. During the time the bottles are increasing in heat, a tea-kettle full of water must be got ready to boil, as soon as the fruit is sufficiently done. If one fire only is used, the kettle containing the bottles must be removed half off the fire, when it is at the full heat required, to make room for boiling the water in the tea-kettle. As soon as the fruit is properly scalded, and the water boiling, take the bottles out of the water one at a time, and fill them within an inch of the cork with the boiling water out of the tea-kettle. Cork them down immediately, doing it gently, but very tight, by squeezing the cork in; but you must not shake them by driving the cork, as that will endanger the bursting of the bottles with the hot water: when they are corked, lay them down on their side, as by that means the cork keeps swelled, and prevents the air escaping out: let them lie until cold, when they may be removed to any convenient place of keeping, always observing to let them lie on their side until wanted for use. During the first month, or two, after they are bottled, it will be necessary to turn the bottles a little round, once or twice in a week, to prevent the fermentation that will arise on some fruits from forming into a crust, by which proper attention the fruit will be kept moist with the water, and no mould will ever take place. It will also be proper to turn the bottles a little round once or twice in a month afterwards.

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* Fahrenheit's.

Having laid down the method of preserving fruit without sugar, in as clear and concise a manner as possible, I will recapitulate the whole in a few words, which may be easily remembered by any person. Fill the bottles quite full with fruit. Put the corks in loosely. Set them in a copper, or kettle of water. Increase the heat to scalding for about three quarters of an hour; when of a proper degree, keep at the same half an hour longer. Fill up with boiling water. Cork down tight. Lay them on their side until wanted for use.

It may be said as an additional reason as well as cheapness, for using wine or porter bottles, instead of gooseberry, is the difficulty of obtaining them, even at any price, in some parts of the country; and indeed they are equally useful for small fruit, and answer the purpose quite as well, excepting the little inconvenience of getting the fruit out when wanted for use, which may be easily done by first pouring all the liquor out into a bason, or any other vessel, and then with a bit of bent wire, or small iron meat skewer, the fruit may be raked out. Some of the liquor first poured off, serves to put into the pies, tarts, or puddings, instead of water, as it is strongly impregnated with the virtues of the fruit, and the remainder may be boiled up with a little sugar, which makes a very rich and agreeable syrup.

In confirmation of the foregoing assertions, I now produce twenty-four bottles as samples, containing twelve different sorts of fruit, viz. apricots, rhubarb, gooseberries, currants, raspberries, cherries, plums, Orleans plums, egg plums, damsons, Siberian crabs*, and green-gages—which have all been preserved in the manner above described.

In order to diversify the degree of heat, and time of continuance over the fire, I have done some in one hundred and ninety degrees, and continued them in it for three quarters of an hour, from which experiments it is evident that the heat is too powerful, and the time too long, as the fruit by that degree and continuance is rendered nearly to a pulp†. In the summer of 1807 I preserved ninety-five bottles of fruit, the expense of which (exclusive of bottles and corks) was 1*l.* 9*s.* 5½*d.*; but having some fruit left, it will not be right to judge them at a higher rate than 1*l.* 9*s.*; and allowing 5*s.* for the extra coals consumed in consequence of my not having a conveniency of doing more than seven or eight at a time, and this being done at fourteen different times, it will amount to 1*l.* 14*s.* the average cost of which is nearly 4¼*d.* per bottle, exclusive of the trouble of attending them; but if we estimate their value in the winter season, at 1*s.* per bottle, that being in general as low or lower than the market price, they will produce 4*l.* 15*s.*, but losing one bottle by accident, reduces it to 4*l.* 14*s.* leaving a net profit of 3*l.* on ninety-four bottles; being a clear gain of nearly two hundred per cent. Another great advantage resulting from this statement will appear by making it an article of store for shipping, or exportation; and I shall

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* Apples and pears may be done for shipping, &c.

† Some of these samples of 1807, were done in 180 and 190 degrees.

submit a few ideas tending to promote such a beneficial object by doing it in large quantities; for which purpose sufficiently extensive premises must be fitted up, with a proper number of shelves, one above another, at a distance of about five inches.

The vessel for scalding the fruit in, should be a long wooden trough of six, eight, or ten feet in length, two or three in breadth, and one in depth, fitted with laths across to keep the bottles upright, and from falling against one another; this trough of water to have the heat communicated to it by steam, through a pipe from a closed boiler at a little distance. The boiling water wanted to fill the bottles with, may be conveyed through a pipe and cock over the trough, by which arrangement, many hundreds of bottles might be done in a short time. It may be prudent to observe that this idea is only speculative, not having been actually practised, but at the same time seems to carry with it a great probability of success, and worthy the experiment.

It remains now that I state some reason or object for troubling the Society, whom I have taken the liberty to address, with these communications. The first is a desire of publicity, sanctioned by their investigation of the experiments made for preserving fruit without sugar, thereby lessening the expense attending an object of so much public benefit and utility. The second arises from a personal or private consideration; but on this subject I shall only observe, that I wish to throw myself entirely on that protection which has ever characterized the liberality of the Society; and that I shall feel highly honoured if they conceive what I have communicated deserving any mark of their favour.

I am, Gentlemen,

Your most obedient humble servant,

THOMAS SADDINGTON.

Wood Street, London,
January 1, 1808.

To the Society of Arts, &c.

XXXVI. *Me-*

XXXVI. *Method of raising large Stones out of the Earth. By Mr.* ROBERT RICHARDSON, *of Keswick, in Cumberland**.

GENTLEMEN,

I, ROBERT RICHARDSON, of Keswick, in the parish of Crosthwaite, and county of Cumberland, beg leave to inform you, that I have found out a method of taking large self-stones out of the ground in a very expeditious manner, and that by this means two men will take as many stones out of the ground in one day, as would require twelve men in the usual way of blasting, and afterwards using large levers, &c.

Where stones of from two to four tons each are to be taken up, two men will raise as many as twenty men in the usual way. The work is done by the power of a tackle, but by my method of fixing the tackle to the top of the stone, by the plug which I have invented, it will hold till the stone is pulled out of the ground, and laid upon the surface, or upon a carriage, if required, all which can be done in a very little time.

Stones of four tons weight, or upwards, may be taken out of the ground within the time of five or ten minutes, by two men, without any earth or soil being previously taken from around them, or without any digging with hacks or spades. J. C. Curwen, esq., of Workington, has seen and approved of my performance with this invention, and if the Society should think it deserving of a premium, it would ever be gratefully acknowledged by,

Gentlemen, your most obedient humble servant,

ROBERT RICHARDSON.

Keswick, Feb. 8, 1808.
To the Society of Arts, &c.

DEAR SIR,

I CANNOT suffer Mr. Richardson's letter to be sent to the

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* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1808.——The silver medal of the Society was voted to the inventor, and one of the implements is preserved in the Society's Repository.

Society without adding a few lines concerning it. I can bear ample testimony to the ease with which the largest self-stones are lifted by his method. I have seen one upwards of five tons lifted by four men. One of the plugs is sent for the inspection of the Society. There is no difficulty in cutting the hole to receive it, the only care is not to make it too large. It is difficult to explain the theory of its action; the least stroke laterally disengages the stone. In many situations it is likely to be of great use, not only in drawing stones out of the ground, but in making weirs and embankments, where the stones are only to be lifted a moderate height.

One of my farmers in Westmoreland has made great use of one, and speaks of it in high terms. I have exhibited it to numbers of persons, who could not believe its power till they saw it tried.

Mr. Richardson submits its examination to the Society, and I conceive it will be very useful and beneficial in cases of new inclosures of land. I do not think it would answer for soft stones, or be safe to use for raising stones in buildings, it being so easily disengaged by any lateral blow. By adding wheels to the tackle machine, or having it upon a sledge, a great deal of time and trouble would be avoided. I purpose to employ this method next summer in making an embankment against the sea; the facility it will give in raising and removing large stones, will expedite the work greatly. If any further certificates of the performance of this plug be required by the Society, I will with pleasure transmit them to you. I will answer for its extracting any stone not exceeding five tons weight out of the ground, without any previous moving of the earth; and it is of importance to preserve large stones entire.

I am, with respect, dear sir,
your obedient humble servant,
J. C. Curwen.

Workington Hall,
Feb. 19, 1808.

To C. Taylor, M.D. Sec.

SIR,

I AM favoured with your letter, desiring my opinion of the utility of the iron plug invented by Robert Richardson, of Keswick. That which I use is about six inches long, and one inch and a quarter in diameter; it requires a hole of its own size, only two inches deep; the plug is to be driven in a little short of the bottom, and will raise a stone of six or eight tons, with the assistance of three men, in the course of ten minutes after the hole is prepared; and I do not hesitate to say, that three men, thus furnished, will clear the ground of large stones in less time, and more effectually, than twelve men by any other method yet come to my knowledge. The plug should be made of good beaten iron. The simplicity and cheapness of the whole apparatus is a great object, as a good plug of the size I use will only cost two shillings and sixpence. I am fully of opinion, that by adding more and stronger ropes and pulleys, work might be done by it to an amazing extent. I have reaped great advantage in my farm from the aid of the iron plug, and, in justice to the inventor, am happy in thus vouching for its extreme usefulness. Several of my respectable neighbours have experienced the aid and benefit of the above instrument, and will vouch, if required, for the truth of the above statement.

I am, sir,
your truly obedient servant,
ROBERT WRIGHT.

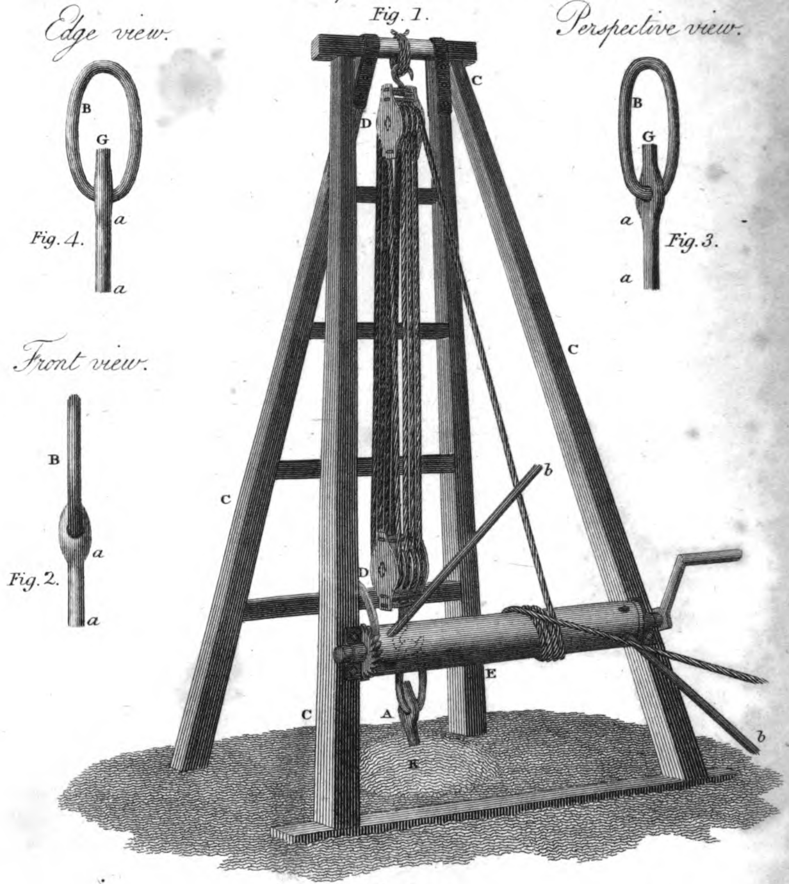
Rose Gill Hall, near Shap, Westmoreland,
May 9, 1808.

To C. TAYLOR, M.D. Sec.

Reference to the Engraving of Mr. RICHARDSON'S *Invention for raising large Stones out of the Earth. See* Plate VI. Figs. 1, 2, 3, and 4.

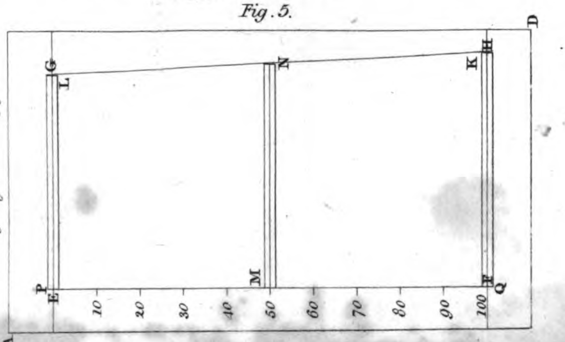
Fig. 1, K, shows the upper part of a stone nearly buried in the earth, having a hole made in it three inches and a half deep, and one inch in diameter, by means of a miner's jumper; the cylindrical tail of the plug *a*, figs. 2, 3, and 4, which is of the same size, is driven fast into it, by means of a hammer applied upon the head of the plug at G. This plug,

Mr. Richardson's method of raising large Stones out of the Earth.



Mr. J. Gough's Hygrometer.

Fig. 5.



J. Farey Junr. delin.

S. Porter sc.

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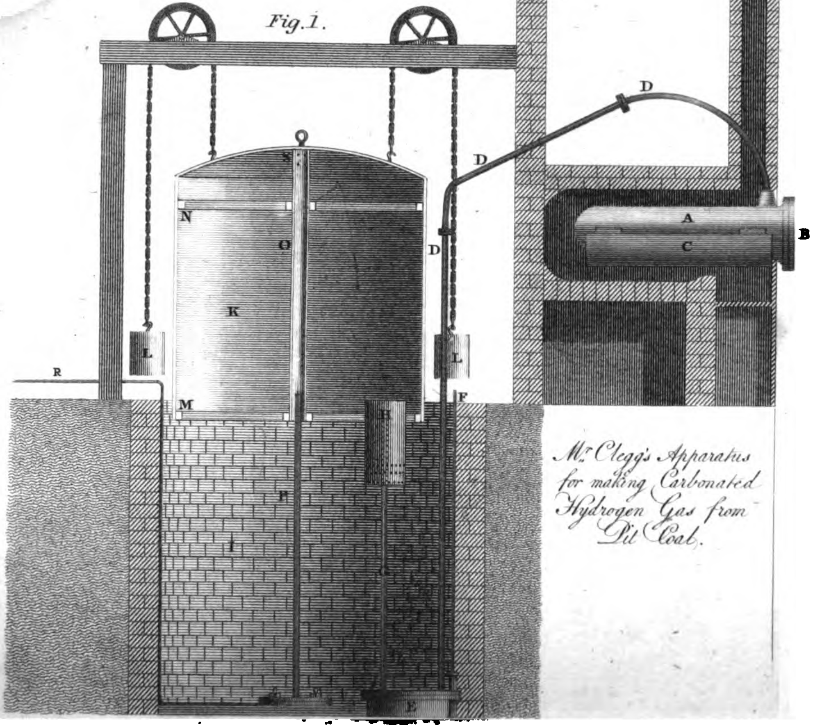
Phil. Mag. Vol. XXXIII. Pl. V.

Fig. 1.



Mr. Clegg's Apparatus for making Carbonated Hydrogen Gas from Pit Coal.

Horizontal Section of the Gazometer at the Bottom part.

Fig. 2.

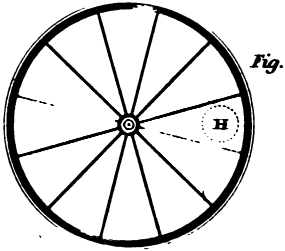


Fig. 5.

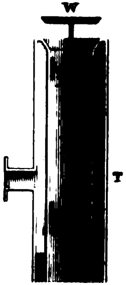


Fig. 4.

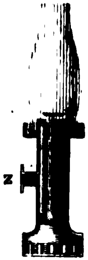
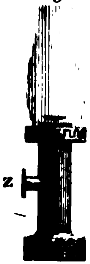


Fig. 3.



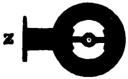


Fig. 0.

131 J. Farey Junr. delin.

S. Porter sculp.

plug, in its whole length, is nine inches, and has a hole made in its broad part H, through which the oval iron ring B passes easily, and on which the plug can move backwards and forwards, when the ring is hung upon the hook of the lower pulley block of the lifting tackle. CCCC represent the four legs or frame-work of the quadrangle, D a five-fold tackle, with blocks ten inches in diameter; E a roller seven inches in diameter, turned by two long iron levers bb; the handle I is used as a safeguard, and to assist to regulate the power of the levers. In fig. 1, the plug A is shown fixed in the stone K, ready to draw it out of the ground, by means of the lifting tackle.

N. B. The hinder legs of the quadrangle are made to close in between the fore legs, for the convenience of carriage.

XXXVII. *Description of an Apparatus for making Carbonated Hydrogen Gas from Pit Coal, for the Purpose of lighting Factories therewith. By Mr.* SAMUEL CLEGG, *of Manchester* *.

DEAR SIR,

WHEN your son was in Manchester, he called to see my nephew's, Samuel Clegg's, improved gas lights, and was desirous to have a plan of his method, which my nephew promised to him, and I undertook to get it conveyed to you. I have, accordingly, taken the opportunity of sending to the Society of Arts, &c., a plan and explanation of his apparatus.

He lighted a large factory in Yorkshire some years ago upon this principle, and has since lighted some buildings in this neighbourhood, and I believe he is the first person who succeeded in rendering these lights free from the offensive smell which generally accompanies them. My nephew served an apprenticeship to Messrs. Boulton and Watt, of Birmingham, in the steam-engine business, in which he is now engaged here on his own account, and has made considerable improvements in their construction.

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* From *Transactions of the Society for the Encouragement of Arts, Manufacture, and Commerce*, for 1808.——The silver medal of the Society was voted to the author.

I remain, dear sir, your most obedient servant,

ASHWORTH CLEGG.

Manchester, May 18, 1808.

To C. TAYLOR, M.D. Sec.

SIR,

YOUR esteemed favour I have received, and, according to your request, have sent you a fuller explanation of the gazometer and lamp, accompanied with further drawings.

A gazometer, containing seven hundred cubical feet of gas, weighs about twenty hundred weight, and costs about two pounds ten shillings the hundred weight.

The whole of an apparatus complete, capable of supporting forty lamps for four hours, each lamp affording light equal to ten candles of eight in the pound, will cost about two hundred and fifty pounds. Each lamp consumes six cubical feet of gas per hour. I am happy to find that the Society have honoured my communications with their attention; and I remain, with great respect,

Sir, your most obedient servant,

S. CLEGG.

Manchester, Aug. 12, 1808.

To C. TAYLOR, M.D. Sec.

Reference to Mr. S. CLEGG's *improved Apparatus for extracting Carbonated Hydrogen Gas from Pit Coal.* See Plate V. Figs. 1, 2, 3, 4, 5, and 6.

In fig. 1, *A* shows the cast iron retort, into which are put the coals intended to be decomposed by means of a fire underneath it, the heat of which surrounds every part of it, excepting the mouth or part by which the coals are introduced. The lid or iron plate *B*, which covers the mouth of the retort, is ground on air tight, and fastened by means of a screw in the centre; *C* is a shield or saddle of cast iron, to preserve the retort from being injured by the intensity of the fire underneath it, and to cause it to be heated more uniformly. *DDD* represents the cast iron pipe which conveys

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veys all the volatile products of the coal to the refrigeratory of cast iron *E*, in which the tar, &c., extracted from the coal is deposited, and from whence they can be pumped out by means of the copper pipe *F*. *G* is the pipe which conveys the gas to the top of the cylindrical vessel or receiver *H;* this receiver is air tight at the top, and consequently the gas displaces the water in the vessel *H*, to a level with the small holes, where the gas is suffered to escape and rise through the water of the well *I*, into the large gazometer *K*. The use of the vessel *H* is pointed out as follows, viz. If the pipe *G* reached all through the water, without passing into the vessel *H*, the gas would not be rendered pure or washed; and if part of the pipe did not rise above the water, the water would have free communication with the tar, besides exposing the retort *A* to a very great pressure, so as to endanger its bursting when red hot. This vessel or receiver *H;* in a large apparatus, is about eighteen inches diameter, and two feet long; the quantity of gas, therefore, which it contains, is sufficient to fill the pipes and retort when cool, and prevents the pipe *G* from acting as a syphon, and exposes the gas to the water without endangering the retort.

When the operation begins, the upper part of the cylindrical gazometer *K*, fig. 1, made of wrought iron plates, is sunk down nearly to a level with the top of the circular well *I*, and is consequently nearly filled with water, but it rises gradually as the gas enters it and displaces the water; the two weights *LL* suspended over pulleys by chains keep it steady and prevent its turning round, otherwise the lower stays *M* of the gazometer would come into contact with the vessel *H*, There are two sets of these stays, one shown at *M*, and the other at *N*.

There is also an iron pipe *O* made fast in the centre of the gazometer by means of the stays, which slides over the upright pipe *P*, by which contrivance the gazometer is kept firm and steady, when out of the well; it likewise prevents the gas from getting into the cast iron pipe *P*, and the copper pipe *R*, any where but through small holes made in the pipe *O* at *S* at the top of the gazometer, where the gas is perfectly transparent and fit for use.

The pure gas enters the tube *O* at the small holes made in its top at *S*, and passes on through the tubes *P* and *R* to the lamps, where it is consumed and burnt.

The seams of the gazometer are luted to make them air tight, and the whole well painted inside and out, to preserve it from rust.

Fig. 2 shows a horizontal section of the lower hoop of the gazometer *K* at the part *M*, with its stays or arms, and the manner in which the iron pipe *C*, before described at fig. 1, sliding on the tube *P*, passes through the ring in the centre of the hoop; a horizontal section of the receiver *H* appears therein.

Fig. 5 shows a section of one of the gas lamps; the space between the outer tube *T* and the inner tube *V*, is to be filled with gas supplied by the pipe *R*, shown in fig. 1, where a stop cock is inserted for adjusting the flame, which gas passes through a number of small holes made in the outer edge of a circular plate shown at fig. 6, which unites the tubes *T* and *V* at their tops. *V* is the inner tube which conveys the atmospheric air into the centre of the flame; the upper part of this tube is made conical, or widening outwards, to join a circular plate with holes in it, a horizontal view of which is shown at fig. 6. *W* is a button, which can be placed at a small distance above the mouth of the lamp, and its use is to convey, in an expanded manner, all the air which rises through this tube to the inner surface of the flame, which assists the combustion very much; this button may be set at any convenient distance above the tubes of the lamp, as it slides in the cross bars *XX*, by which it is supported in the inner tube.

A current of air also passes between the glass tube or chimney and the outer tube *T*, through holes made in the bottom of the glass-holder, as in Argand's lamps; this surrounds the flame, and completes its combustion, as explained by the view, fig. 3, and section, fig. 4, which have a glass upon each. *ZZZZ*, figs. 3, 4, 5, and 6, show the tube through which the lamp is supplied with gas from the pipe *R*, fig. 1.

XXXVIII. *Re-*

XXXVIII. *Report of Dr.* M. Garthshore *and* Patrick Colquhoun, *Esq., to the Society for bettering the Condition of the Poor, to whom it was submitted to consider the Expediency and Practicability of establishing an Experimental Dispensary in the Metropolis, comprising in its Structure a Dietetic Regimen for debilitated Patients.*

Before any accurate opinion can be formed of the utility, necessity, and practicability, of adding a dietetic to the medicines generally administered to the poor at the different dispensaries in the metropolis, it may be useful to detail a number of prominent facts, which either bear directly or collaterally on this subject, and which are necessary to assist the mind in forming a correct judgment.

According to the parliamentary returns of the year 1803, it appears that the number of poor persons relieved in that year in the metropolis, comprehending all the parishes within the bills of mortality, besides Marylebone, St. Pancras, Paddington, Kensington, Chelsea, and Islington, (including a population, according to the parliamentary returns of 1801, amounting in the whole to 846,845 persons,) was 86,120.

Of these 86,120 poor persons relieved,

	14,746	were maintained in sixty workhouses, at the yearly expense of 14*l*. 8*s*. 3½*d*. per head.			
Average of both classes 2*l*. 0*s*. 2¼*d*. a year.	21,877	were relieved out of workhouses, at the expense of about	£2	15	0
	33,187	were occasionally relieved—at the expense of about -	1	5	0
	16,310	were relieved, not being parishioners, supposed vagrants,	0	2	0
Total	86,120				

The number of children under fourteen years of age are nearly equal to the adults who have received relief. The workhouses (sixty in number) are generally full during the winter months, and the greatest number that can be accommodated does not exceed 17,000 men, women, and children.

The number of distressed objects who do not receive any parish relief, but who are supposed, in many instances, to require it as much as those who are relieved, may be estimated at about 20,000 men, women, and children.

It will be seen from the above abstracts, that the permanent out-door relief seldom averages above 2*s*. to 2*s*. 6*d*. per week, while the occasional relief is infinitely less, which is barely sufficient to pay the weekly rent of a miserable half-furnished lodging.

It will also be seen, that many thousand cases may occur, where half-famished families cannot obtain an asylum in their parish workhouse for want of room. And that the proportion of those who are relieved at their own dwellings, is nearly *four* to *one*.

Hence it follows as a clear proposition, that there ever has been, and always must be, a very large proportion of the poor of the metropolis, who can derive no benefit from the maintenance afforded in the parish workhouse: and that the pittance allowed in money, can afford little for food, where a family is broke down by sickness, and their only property (the labour of their hands) no longer effectual or productive. Hence, in such cases, the pawnbroker assists in filling up the chasm until their little all is exhausted, and they are not only without food, but also deprived of their apparel and bed clothes.

To relieve this numerous class, who are subject to so many casualties, reducing them to a state of extreme indigence, benevolent individuals have founded hospitals and dispensaries in different parts of the metropolis; but many of the hospitals are ill endowed; and from a deficiency of funds, they are not adequate to the relief of one-tenth part of the patients who would be glad to become inmates during sickness and disease.

The dispensaries are more numerous. It appears from an authentic return from thirteen of these establishments in different parts of the metropolis, that, in the course of the year 1808, medicines were dispensed to 28,154 patients, at the expense of about 1200 to 1500*l.* for drugs, and perhaps about 2000*l.* for house-rent, taxes, salaries, and other expenses—in the whole, between three and four thousand pounds a year. These nearly comprehend all the dispensaries in the metropolis.

But it requires little investigation to convince the mind, that drugs alone will not restore an enfeebled body to health, where the cause of the disease originated in the want of nourishing diet. On the contrary, they are often pernicious, unless accompanied by a dietetic regimen, which is out of the reach of a considerable proportion of those distressed objects who become patients to the different dispensaries. There, every medicine is to be found, but that alone which in many cases is to effect a cure. Their bodies are wasted, and must be restored by nourishing food. Their recovery depends on this; but it is not attainable—it is not to be found in their miserable dwellings; and the workhouse is shut against them—it is already full; and the hospitals are also inaccessible.

That this evil exists, in a great degree, in the metropolis, must be evident from the state of the poor, which has been already explained, and which rests on the solid foundation of parliamentary documents.

That such also is the state of many patients who are relieved at dispensaries, every candid medical man who attends these dispensaries will admit. If the evil therefore exists, and if its magnitude is as great as the facts stated afford the strongest grounds to suppose, a question will arise among those who are benevolently employed in their laudable endeavours to better the condition of the poor in the metropolis—In what way a remedy can be applied?—a remedy which shall restore parents to their families, and children to their parents, who must otherwise drop into the grave.

In suggesting new and untried objects, difficulties naturally occur to the mind, which often vanish on a patient investigation; and such it is earnestly hoped will be the case on the present occasion.

The dispensaries at present administer those medicines which are most generally applicable to that part of the community who are in easy circumstances. To adapt them to the poor, there ought to be superadded, a certain moderate proportion of spirits for cordials—strong porter—soups—and also flannel waistcoats and shifts and shirts. These will avail more in many disorders arising from scanty food, than all the materia medica. Nor will the difficulty of preparing and dispensing these auxiliaries be so great, or the expense so formidable, as may appear to those who have not minutely investigated the subject in detail. The dietetic is proposed to be dispensed as medicine, not as food. It will make a part of the physician's and surgeon's prescription, where, upon due inquiry, and according to the nature of the case, such auxiliary aid, together with the flannel garments, are found to be necessary to give effect to the drugs. Both are to be dispensed in small portions, and only to those who actually require such aid, and cannot otherwise obtain it. The soup to be prepared and taken in the kitchen of the dispensary, on the order of the medical attendant, only in extreme cases. The dietetic is capable of being so systematized as to prevent not only the shadow of abuse, but also at no additional expense of servants; and the materials composing it, and all the other auxiliaries will cost infinitely less than can be supposed at first view, as will appear from the following statement :

Estimate of the Expense of an Experimental Dispensary, with the Dietetic Auxiliary.

House-rent and taxes - - - - -		£100 0 0
Apothecaries' salary - - - - -		80 0 0
Servants' wages, &c. - - - - -		40 0 0
Coals and candles - - - - -		40 0 0
Drugs - - - - - - -		70 0 0
Spirits for cordials - - - - -		25 0 0
Malt liquor of the best quality - -		40 0 0
Meat, consisting of legs and shins, and clods and stickings, for soup -	£45 0 0	
Potatoes for potatoe soup, &c., vegetables, barley, &c., about - -	15 0 0	
Flannel waistcoats and shirts and shifts	25 0 0	85 0 0
		£480 0 0

The usual expense of medicines is here reduced, because in many instances the dietetic will be substituted for drugs, which would otherwise be administered, producing at present little or no benefit to debilitated patients, whose disorders have been chiefly occasioned by deficient nourishment.

The soups to be of two kinds:—Beef tea for debilitated patients; and a stronger broth mixed with vegetable substances for those who are in a state of convalescence, and can bear a stronger diet. Not more than a pint of any of the two soups will probably be ordered by the physician or surgeon to any one patient, which must be taken in the common kitchen of the dispensary. The cost of a pint of either kind of soup cannot be estimated at more than twopence, (including the expense of fuel,) and this to be given in lieu of a composition of medicine which would probably cost double that sum. Two common boilers, such as are used in private families, will be sufficient to prepare the soups for each day's delivery; and admitting that twenty patients (although an opinion prevails that ten will be the utmost number) require soup, the whole quantity to be prepared on any one day will not exceed four gallons, and the total expense will be 3*s*. 4*d*. Other patients may require strong porter—a pint of which (in a pint bottle) is to be delivered to the patient on the prescription of the physician or surgeon; and supposing ten pints to be issued in one day, the expense will not exceed 2*s*. 6*d*.—the patient to bring back the bottle to be again filled, or not, according to the prescription of the medical gentlemen. It has already been observed, that the common kitchen of the house will be amply sufficient for every purpose; and the design is capable of being so systematized as to prevent the possibility of abuse. The labour to the cookmaid will be next to nothing. The spirits will be made up in cordials, by the direction of the physicians, and administered to such patients as may require this species of assistance in order to promote their recovery. It will be delivered in the disguised state of a drug, to be taken at different times, under circumstances where no abuse can possibly take place, at the residence of the patients.

Supposing 4,000 pints of soup to be dispensed in a year, at 2*d*. a pint -	£33	6	8
3,000 pints of strong porter dispensed in a year, at 3*d*. a pint -	37	10	0
Total expenses	£70	16	8

Considering this limited dietetic in the light of new and more appropriate medicines, nothing in the general œconomy of the system can be supposed to experience any change. One prescription from the physician or surgeon goes to the apothecary, and another to the kitchen. Nothing is in the smallest degree disturbed, and the utmost regularity would prevail.

Under a self-evident presumption, that this dietetic is to save the lives of many individuals, who would otherwise sink under their complaints; and, by thus giving effect to the power of the medicines, preserve many useful lives,—it is scarcely possible for the human mind to devise any scheme where so much good is likely to be done at so small an expense. Nor is there any way in which the condition of the sick poor in the metropolis can be so much improved; since the success of an experimental dispensary, with a dietetic auxiliary, upon the plan now proposed, (as to which there can be no doubt,) would be the means of extending the same system to the other dispensaries in the metropolis; and thereby contribute to the recovery of many hundred poor persons in the course of a year, to whom, for want of a small portion of nourishing food applied at a critical moment, medicines can be of little use in effecting a cure.

For these and other reasons which could be adduced, the reporters are decidedly of opinion, that an experimental dispensary, upon the plan now proposed, would prove an incalculable benefit to the poor; and that it highly merits the patronage and countenance, not only of this society, but of the public at large.

M. GARTHSHORE.
P. COLQUHOUN.

London, Feb. 3, 1809.

At a Meeting of the Committee of the Society for Bettering the Condition of the Poor, held at Mr. Hatchard's, Piccadilly, on Friday the 3d of February, 1809,

THE LORD BISHOP OF DURHAM IN THE CHAIR,

Resolved,—That this meeting do unanimously approve of the suggestions offered in this Report, and will afford every countenance and assistance in promoting the experimental dispensary with an auxiliary dietetic, upon the plan which has been proposed.

Resolved,—That the said Report be forthwith printed, and generally circulated among the members of this society, in the expectation that their aid and countenance will be afforded in carrying the design into effect.

Resolved,—That one hundred copies of the said Report be sent to each of the dispensaries in the metropolis.

Resolved,—That one hundred copies of this Report be presented to Dr. Herdman, of Old Broad Street, London, who first suggested and elucidated the plan for improving dispensaries by a dietetic auxiliary, and the medical treatment of the diseased poor, in a printed letter addressed to the president, vice-presidents, and the other members of the committee of this society.

S. DUNELM, President.

XXXIX. *Memoir upon the Vineyards and Wines of Champagne in France: Written in answer to certain Queries circulated by M.* CHAPTAL. *By M.* GERMON, *of Epernay.*

[Concluded from p. 150.]

Is Grafting attended with Advantage?

FIFTY years ago they used to graft the vines on the mountains, and they generally obtained very fine vines with large fruit. This plan has now been abandoned, because it has been discovered that a grafted vine does not last so long as an ungrafted one, and the grafted vine is always more tender and delicate; besides, it produces a poorer kind of wine.

How

How many Dressings are given to the Vines previous to the Vintage?

The first dressing, which is called *becherie* (hoeing), is given immediately after the frosts have disappeared.

In general, as soon as the bud of the vine makes its appearance the women proceed to prune, and the men follow with the first dressing. This is a pernicious system; but the prejudices of the proprietors have not yet given way to the counsels of men of science.

The vines are pruned at the same time with the first hoeing; but this method is not practised in the Marne district, where they prune subsequently to the hoeing: it frequently, however, saves the prunings from the effects of the frost, and presents a resource to the proprietor if the vines have suffered from this accident.

Two other dressings are afterwards given, one in June and the other in August; but some proprietors, who are jealous of the good qualities of their vines, give them a third dressing in September.

What are the Processes employed in gathering and pressing the Produce of the Vintage?

In order to make red wine,—when the fruit is perfectly ripe, the black grapes only are carefully picked and gathered. The white grapes are laid aside, as well as those red ones which are not ripe; and these are afterwards made into wine of an inferior quality. The ripe red fruit, when thus separated, is put into panniers, or small wooden boxes called *barillets* or *cuvelets*, and conveyed on the backs of beasts of burden to the pressing-place: here they are pressed by small portions at a time, and the juice then put into a tub to ferment. In performing this operation some proprietors employ an utensil called a martyr, which is very useful. This is an oblong coffer, less than the diameter of the fermenting-tub, and about a foot or eighteen inches high. This coffer rests upon beams placed across the fermenting-tub, and its bottom and sides are pierced with holes in such a manner as to allow the expressed juice of the grapes to flow through into the tub.

How

How long is the Wine allowed to ferment?

It would be difficult to fix any precise time for the duration of the fermentation; this depends entirely upon the nature and maturity of the fruit, and upon the influence of the atmospheric air. Grapes gathered in the morning will more slowly go into fermentation than those which have been gathered after noon-day in fine weather:—mists, rains and hoar-frosts, all retard fermentation more or less.

In some years, three or four days are sufficient for producing a fermentation sufficient for preparing the fruit for the press:—in other years, ten, fifteen, and even twenty days are required.

By what Sign is it ascertained that the Fruit has attained a proper Degree of Fermentation?

We cannot assign any certain symptoms that the wine has sufficiently fermented, as the period proper for placing the bruised fruit into the presses depends upon various causes; upon the pleasure and experience of the proprietor, and upon the quality and colour which he wishes to give to his wine. Some place the fruit in the press at the strongest degree of fermentation, and others when it has slackened.

After the fermentation begins, in order to hasten it, they squeeze down the fruit in such a manner as to keep the must always uppermost: poles armed with spikes are used for this purpose; or, what is better, some strong workmen descend into the vat and tread down the fruit: the fermentation thus becomes more equal and more general; and when it has proceeded far enough, the must is carried to the press and the wine is made.

In ordinary years, when a lighted candle cannot be held over the tub without going out; when the grapes and husks ascend to the top, notwithstanding their being repeatedly pressed down; when the must undergoes a kind of ebullition; and lastly, when the colouring particles are sufficiently decomposed to satisfy the wishes of the proprietor,—it would be dangerous to push the fermentation any further, as in

that

that case the wine might assume a dry and hard taste which even time could not correct, particularly in Champagne wines, which are prized on account of their pleasantness and lightness. The most consummate experience is sometimes unsuccessful in the above operations, and there has been no instrument yet invented which can be depended upon.

Is it advantageous to mix the extractive Liquor of the Tubs with that which is produced by pressing?

This may be answered in the affirmative, with respect to the whole of Champagne;—and it is very advantageous for the following reasons:

1st, The wine made from the tub would be paler in colour and more delicate than that which is expressed from the husks.

2dly, The wine which came from the press only, would be harder, stronger, and redder, than the other; so that from the same tub we should certainly have two different kinds of wine:—The mixing of them is therefore indicated by experience, and it is at all times necessary to have wines of an equal quality.

Is it advantageous to bruise the Stones of the Grapes?

This operation depends upon the season, and upon the ripeness and nature of the fruit. When the fruit is small and the stone large, or when the fruit has not acquired all its maturity, the stones should be bruised.

When the fruit is full and well grown, when the season has been rather dry than humid, this operation may be omitted. It has been ascertained, however, that the strong and rough taste of the stones is necessary, as one of the constituent parts of wine.

To what Accidents are Red Wines liable when in the Casks?

The accidents to which red wines are liable, are yellowness, muddiness, and a wormwood taste. These accidents happen when the wines are kept in badly-aired cellars, or when the fruit has been damaged during the vintage season by frosts or continual rains.

How are the Red Wines managed?

When the red wine which comes out of the tub, and that which has been expressed from the husks, are well mixed together in a vat procured for the purpose, the whole is poured into new puncheons previously rinced with hot water; but they are not filled at once, because the wine always ferments for a few days longer:—as the fermentation ceases they are filled and bunged up, leaving a small spiggot in the bung in order to allow the gas to pass out: when the fermentation has entirely ceased the puncheon is hermetically closed.

About the end of December, and if possible in dry weather, the wine is drawn off and freed from the greatest part of the lees.

About the middle of May, before the warm season commences, the wine is again drawn off clear. Before putting the puncheons into the cellar they are furnished with new hoops, where they are kept during the summer, or till they are sold, otherwise their contents would be spoiled.

What is the Method of clarifying Red Wines?

This consists in drawing off the wine a third time. The whites of five or six fresh eggs are diluted in a chopin of water; and this quantity is sufficient for each piece or puncheon containing 240 bottles.

There are only 200 bottles in a puncheon of white wine.

The whites of eggs are well beaten up and then thrown into the puncheon, the contents of which are then briskly stirred up by a cleft stick.

This operation is performed previous to bottling the wine or sending it off to a market.

At what Age should Red Wines be bottled?

In general, the red wines of *Haute Montagne* are bottled in the month of November succeeding the vintage, *i. e.* thirteen months afterwards. This season of the year is fixed

upon

upon because all germination has ceased, and Nature is in a state of perfect repose; thereby all fermentation is avoided; and red wines may therefore be safely bottled from the first of October until the end of December: at any other season great inconveniences would arise; for I know of nothing worse than red wine bottled in spring time; it retains a slight degree of fermentation, and is very disagreeable to the taste.

There are some excellent and generous wines which can remain three or four years on their lees: of this description are the wines of St. Thierry.

How long will Red Wines keep in Bottles?

The more body and spirit the wine has, the better is it preserved in bottles: the more tender, delicate, and light it is, the more difficultly is it kept.

This is the reason why the wines of Mailly, Chiny, Chenay, and Hermonville, keep worse than those of Verzenay, Bouzy, and Verzy; and these last worse than those of St. Thierry. To conclude:—We may safely venture to say, that the best red wines of Haute Montagne will keep in good cellars for six, eight, ten, and twelve years.

What Degree of Temperature, according to Reáumur's Thermometer, ought the Cellars to have?

The cellars in Champagne are from 25 to 40 feet in depth, particularly those which are dug in beds of chalk, and in which it is necessary to dig low, in order to obtain such a solidity of earth above, as to render an arch unnecessary. It results from experiments made by Messrs. Dubois, merchants at Rheims, that several good thermometers placed in various situations in their cellars, always marked five degrees below the temperature of the atmosphere. The variations between winter and summer were not half a degree, and could not be noted.

What is the Cost of an Acre of Vineyard?

FIRST CLASS.

	Livres.
In Haute Montagne -	2000
In Basse Montagne -	1000
In St. Thierry - -	900

SECOND

SECOND CLASS.

		Livres.
In Haute Montagne	-	900
In Basse Montagne	-	600
In St. Thierry	- -	300

The convent lands, and what is called Clos St. Thierry, are not taken into this computation.

What is the annual Expense of Cultivating an Acre of Vineyard, including the Expense of Vintage and of Pruning?

		Livres.
To the vine-dresser	- -	50
Props	- - - -	18
Mending them, and carriage, &c.		40
Prunings and contributions	-	24
Four puncheons	- -	40
Expense of vintage	- -	28
		200

GENERAL OBSERVATIONS.

We have only mentioned the culture of vines in general, without detailing those of the high and low grounds separately. There are many vineyards, however, and particularly in St. Thierry, where the greater part of the vines is always raised to the height of about five feet, and supported by props of oak, six feet high, and an inch in diameter. This kind of vine can only answer in strong and vigorous ground.

The difference between the culture of the high and low vineyards, consists in the shaping, tyeing, and pruning.

1st, Shaping consists in choosing from the sucker the best stalk, in preference to others which are cut off, and all the small collateral shoots are lopped off.

2dly, The tyeing is effected when the sap is most abundant, and the bud already developed: the above single stalk is bent like a hoop, and tied to the prop in two or three places.

3dly, Pruning consists in reinserting into the earth, and into small and long holes, every ten or fifteen years, the old sucker, upon which three or four stalks are left, which are also buried in the earth; and they send out an excellent

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plant for the ensuing year. This operation is called *ravallement,* and is very different from the pruning practised in the department of the Marne.

An intelligent proprietor, who has a large extent of vineyard, should bury some vines (*raveller*) every year, in order to have a sure and constant supply of plants for replanting.

The methods of treatment are in every respect the same with high and low plants.

XL. *On the Affinity existing between Oxides of Carbon and Iron.* *By* David Mushet, *Esq.*

In my late communications to the Philosophical Magazine, a number of experiments were adduced to exhibit the universal diffusion of carbon, and to convey a tolerably correct idea in what proportions it enters into the composition of animal, mineral, and vegetable substances.

The affinity that exists mutually betwixt iron and carbon is every day manifested as the basis of one of our greatest national manufactures: in particular, in the various modifications of cast iron, steel, plumbago, &c.

Any inquiry that has for its object the investigation of those means, and of their peculiar modes of formation, to which we exclusively owe the existence of the most widely diffused and most useful metal that has hitherto been known in civilized society, commands attention, not only as a matter of curiosity, but as an object of the highest importance.

The natural mutual affinity of iron and carbon is such, that they may be reciprocally used as tests and agents of each others existence respectively; and upon this principle, chiefly, the experiments which are to follow were performed.

The first object to be ascertained, in pursuing this investigation, was the nature of oxide of carbon, whether in the state of charcoal, coke, plumbago, &c.; and wherein, and in what, it differed from the matter of carbon that existed naturally in the substances from which these were obtained.

If any two oxides of carbon were taken similarly compounded

pounded as to the alloy of foreign matter, and employed under the same circumstances, to revive equal quantities of the same metallic oxide, Would in not be just to infer, that if the quantities of metal were equal, so were the qualities of the oxide; and the reverse if a different result were obtained? And does it not appear equally fair to deduce, that where the greatest quantity of metal is revived, the appropriated oxide is of superior quality? The former deduction holds universally correct; but the latter, if admitted, would often lead into error, particularly where the oxides exist in the state of coal or coke.

I was once of opinion that the carbonating powers of any oxide depended upon the real quantity of combustible matter which it contained; and that the substance found to yield the largest portion of coal or coke, and to contain the smallest portion of ashes, would, every thing else being alike, revive the greatest quantity of metallic oxide; or, in other words, would be found to contain the largest quantity of pure carbon, or diamond: but upon investigating the nature and properties of a variety of carbonaceous oxides, chiefly obtained by the distillation of pit coal, with a view to fix an unerring list whereby to judge of coal fit for iron making, it was found that not only this conclusion was of itself erroneous, but that, in general, the very reverse of this theory took place. It not only appeared that the carbonating powers of the oxide depended upon some other cause, remote from the actual quantity of combustible matter, but that the process of distillation, or of coking, subjected the oxide to new laws, the very reverse of what at first view ought to have taken place. This, then, was at once attributed to the state of oxidation of the oxide; and a direct probability inferred, that that coke or coal that revived the greatest quantity of metallic oxide would be the least oxidated; or, in other words, approach more nearly to the state of diamond.

This theory, however plausible, was found incompatible with practice; and in the event it was found that the coke or coal that became most deoxidated in burning, revived, under precisely the same circumstances, the least quantity of metallic oxide: and from the experiments hereafter to be detailed, it will not appear rash to infer, that this inverse ratio of carbonation proceeded exclusively from an approximation, however remote, to the state of diamond; arising chiefly from a new and more dense arrangement of the particles of the coal, in consequence of parting with a portion of oxygen.

These experiments were arranged and conducted in the following order:—About 50 pounds of oxide of iron were prepared, and thoroughly mixed, that there might not exist any variety of quality arising from different preparations. This was kept during the whole series of experiments in the same temperature, that none of the results might be affected by the moisture of the atmosphere. A parcel of hand-made crucibles, all nearly of the same size, prepared from Stourbridge clay, with ground covers, made so as to form a water-tight joint, were set aside for the experiments. These, from time to time, before the introduction of the mixture, were brought to a red heat: when in this state, the mixture was introduced, the cover was slipped on, and the whole was put directly into the assay furnace. This mode had not only the advantage of facility, but, which in these experiments is absolutely indispensable, prevented the carbonaceous matter introduced from being dissipated by the moisture, which is always contained in the clay when crucibles are put into the furnace in a green state.

The proportions of oxide of iron and oxide of carbon, used in the most of these experiments, were oxide of iron 200 grains, of carbon 15 grains.

These were intimately mixed, and put into a square of thin paper containing about five superficial inches of measurement, and productive exactly of half a grain of charcoal: this, and 14½ grains of the substance to be tried, formed the quantity of 15 grains. So that in all these experiments there is nearly 1-29th of carbon of paper united. When the experiments were directed to comparative views betwixt the raw and coked materials, a quantity of the former was used, that would, by accurate experiment, have formed 15 grains of coal, or prepared oxide of carbon. The paper and mixture were with facility introduced into the crucibles respectively, and the immediate introduction of the cover prevented the most minute contact of air, or dissipation of the subject of experiment. The duration of the crucible and degree and management of the heat in the furnace were scrupulously attended to, and many of the experiments were repeated three times.

The first class of experiments was with various woods, from which the following were selected:

Experiment I.

Charcoal prepared from Walnut, composed of

Oxide of carbon -	96·048
Ashes - - -	3·952
	100 parts.

15 grains of this oxide and 200 grains of oxide of iron, were subjected to fusion, after being carefully mixed, and the result was a metallic button which weighed 36 grains, (equal to 18 per cent.) from the oxide of iron.

It was found, upon a calculation of what this wood lost in distilling, that 76 grains of raw wood would have formed 15 grains of the charcoal operated upon. This quantity of wood was therefore rasped down, and, in a similar manner with the former, introduced into the crucible. The result was

A metallic button weighing - - -	49 grains.
Charcoal of the same wood, revived as above,	36
Increase (equal to 6½ per cent, or 24½ upon the whole,) - - - - -	13

Experiment II.

Charcoal prepared from Elm, composed of

Oxide of carbon -	96·70
Ashes - - -	3·30
	100 parts.

15 grains of oxide from elm and 200 grains of oxide of iron, yielded a metallic button weighing 40 grains (equal to 20 per cent.).

75 grains of the same wood, in the state of raspings, found to be equal to 15 grains of prepared coal, being mixed with 200 grains of oxide of iron, they were fused together, and the result was a metallic button weighing 59 grains. (Equal to 29½ per cent. from the oxide of iron.)

With the charcoal, the quantity revived was	40
Increase (equal to 9½ per cent.) -	19

Experiment III.

Charcoal prepared from Holly,

Composed of oxide of carbon	94·152
Ashes - -	5·848
	100 parts.

15 grains of this carbon and 200 grains of oxide of iron, yielded by fusion a metallic result that weighed 44 grains. (Equal to 22 per cent.)

71·4 grains of raw holly, being equal to 15 grains of charcoal, and 200 grains of oxide of iron, yielded by fusion

A neat metallic button weighing	45 grains.
Revived as above with charcoal	44
Increase (equal to ½ per cent.)	1

Experiment IV.

Charcoal prepared from Scotch Pine,

Composed of oxide of carbon	97·10
Ashes - -	2·90
	100 parts.

15 grains of this charcoal and 200 grains of oxide of iron, being fused together, yielded a metallic button of iron weighing 40 grains, or 20 per cent. 88 grains of raspings of this wood (equal to 15 of charcoal,) and 200 grains of oxide of iron, yielded a button of iron that weighed

34 per cent., or - -	68 grains.
Revived as above with charcoal	40
Increase (equal to 14 per cent.)	28

Experiment

Experiment V.

Charcoal prepared from Beech,

Composed of oxide of carbon	95·20
Ashes - -	4·80
	100 parts.

15 grains of charcoal of beech mixed with 200 grains of oxide of iron, yielded by fusion a metallic button of iron weighing 42 grains, (equal to 21 per cent.)

71·5 grains of raspings of this wood, (found equal to 15 grains of carbon,) being mixed with 200 grains of oxide of iron, there resulted from the fusion of this compound a metallic button that weighed (= to 27¼ per cent.) 54·5 grains.
Revived as above with charcoal - - 42

Increase (equal to 6¼ per cent.) 12·5

Experiment VI.

Charcoal prepared from the American Maple-Tree,

Composed of oxide of carbon	96·140
Ashes - -	3·860
	100 parts.

15 grains of this charcoal and 200 of oxide of iron gave a metallic button weighing 50 grains, (equal to 25 per cent.) from the oxide of iron.

76 grains of this wood (equal to 15 grains of charcoal,) and 200 grains of oxide of iron, yielded by fusion a button of iron, weighing (equal to 30½ per cent.) 61 grains.
Revived as above with charcoal - - 50

Increased, (equal to 5½ per cent.) 11

Experiment VII.

Charcoal prepared from Spanish Mahogany,

Composed of oxide of carbon	96·154
Ashes - -	3·846
	100 parts.

15 grains of this charcoal with 200 grains of oxide of iron, gave by fusion a metallic button weighing 40 grains, (equal to 20 per cent.) from the oxide of iron.

55·5 grains of mahogany was found equal to 15 grains of charcoal. This, in the state of small raspings, was mixed with 200 grains of oxide of iron. The result by fusion was

A metallic button weighing	43	grains.
Revived as above with charcoal	40	
Increased, (equal to 1½ per cent.)	3	

Experiment VIII.

Charcoal prepared from Sallow,

Composed of oxide of carbon	93·865	
Ashes - -	6·135	
	100	parts.

15 grains of this charcoal mixed with 200 grains of oxide of iron, gave by fusion a metallic button which was found to weigh 43 grains, (equal to 21½ per cent.)

79 grains of sallow, being found equal to 15 grains of charcoal, this, in the state of raspings, was mixed with 200 grains of oxide of iron. The compound was fused, and the result was

A button of iron weighing	60	grains.
Charcoal revived only -	43	
Increase, (equal to 8½ per cent.)	17	

Experiment IX.

Charcoal prepared from American Black Beech,

Composed of oxide of carbon	95·169	
Ashes - -	4·831	
	100	parts.

15 grains of this charcoal mixed with 200 grains of oxide of iron, yielded by fusion a metallic result weighing 36 grains, (equal to 18 per cent.)

69 grains of black beech were requisite to form 15 grains of charcoal. These, in the state of raspings, were mixed with

200 grains of oxide of iron and fused together, the result was,

A metallic button that weighed 48 grains.
Revived with charcoal - 36

Increase (equal to 6 per cent.) 12

[To be continued.]

XLI. *Some Circumstances relative to Merino Sheep, chiefly collected from the Spanish Shepherds, who attended those of the Flock of Paular, lately presented to His Majesty by the Government of Spain; with Particulars respecting that great National Acquisition; and also respecting the Sheep of the Flock of Negrete, imported from Spain by His Majesty in the Year* 1791*. *By Sir* JOSEPH BANKS.

Soho Square, Feb. 18, 1809.

SIR JOHN,

AT a time like the present, when Spanish wools, though at a price unheard-of in the annals of traffic, still continue to find a market; thus clearly proving, that their value in the estimation of the consumer is far above any price that has been hitherto offered for them by the manufacturer; and when we must all agree, that the interruption of our trade with Spain may still continue for some time longer, I trust that a paper written with a view to facilitate the production of this valuable article in the United Kingdom, and to communicate some information relative to the important present of Merino sheep lately received by our most gracious Sovereign from the government of Spain, will be interesting to you, sir. I beg the favour of you, in case you shall approve it, to do me the honour of placing it at the disposal of the very useful institution over which you preside with so much advantage to the agricultural interests of this country.

I have the honour to be, sir,
Your obedient and faithful humble servant,
JOSEPH BANKS.

Sir John Sinclair, Bart. President
of the Board of Agriculture.

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* From Communications to the Board of Agriculture.

A CONSIDERABLE part of Estremadura, Leon, and the neighbouring provinces of Spain, is appropriated to the maintenance of the Merino flocks, called by the Spaniards Trashumantes, as are also broad green roads, leading from one province to the other, and extensive resting-places, where the sheep are baited on the road. So careful is the police of the country to preserve them during their journeys from all hazard of disturbance or interruption, that no person, not even a foot passenger, is suffered to travel upon these roads while the sheep are in motion, unless he belongs to the flocks.

The country on which the sheep are depastured, both in the southern and the northern parts, is set out into divisions, separated from each other by land-marks only, without any kind of fences; each of these is called a Dehesa, and is of a size capable of maintaining a flock of about a thousand sheep; a greater number, of course, in the south country, where the lambs are reared, and fewer in the north country, where the sheep arrive after the flock has been culled.

Every proprietor must possess as many of these in each province as will maintain his flock. In the temperate season of winter and spring, the flocks remain in Estremadura, and there the ewes bring forth their lambs in December. As soon as the increasing heats of April and May have scorched up the grass, and rendered the pasturage scanty, they commence their march towards the mountains of Leon; and, after having been shorn on the road, at vast establishments called Esquileos, erected for that purpose, pass their summer in the elevated country, which supplies them with abundance of rich grass; and they do not leave the mountains till the frosts of September begin to damage the herbage.

A flock in the aggregate is called a Cavaña: this is divided into as many subdivisions as there are thousands of sheep belonging to it; each sheep, besides being sear-marked in the face with a hot iron when young, is branded after every shearing with a broad pitch brand, generally of the first letter of the name of the proprietor, and each subdivision is distinguished from the rest by the part of the sheep's body on which this mark is placed.

By the laws of the Mesta, each Cavaña must be governed by an officer called Mayoral; for each subdivision of a thousand sheep, five shepherds and four dogs are appointed. Some of these inferior shepherds obtain the office of Rabadan, the duty of which is to give a general superintendance under the control of the Mayoral, also to prescribe and administer medicines to the sick sheep. At the time of travelling, and when the ewes are yeaning, one or two extra shepherds are allowed for each thousand sheep.

The number of Merino sheep in Spain is estimated by Burgoyne at 6,000,000; these of course must be attended by 30,000 shepherds, and 24,000 dogs at ordinary times, and they find occasional employment for 5 or 10,000 additional persons in the seasons of lambing and of travelling.

In their journey, each subdivision is attended by its own shepherds and dogs, and kept separate as far as may be from all others. The duty of the dogs is to chase the wolves, who are always upon the watch when the sheep are on the road, and are more wily than our foxes; they are taught also, when a sick sheep lags behind unobserved by the shepherds, to stay with and defend it, till some one returns back in search of it. There are besides in each subdivision about six tame wethers, called Mansos; these wear bells, and are obedient to the voices of the shepherds, who frequently give them small pieces of bread: some of the shepherds lead, the Mansos are always near them, and this disposes the flock to follow.

Every sheep is well acquainted with the situation of the Dehesa to which its subdivision belongs, and will at the end of the journey go straight to it, without the guidance of the shepherds. Here the flock grazes all the day under the eyes of the attendants: when the evening comes on, the sheep are collected together, and they soon lie down to rest; the shepherds and their dogs then lie down on the ground round the flock, and sleep, as they term it, under the stars, or in huts that afford little shelter from inclement weather; and this is their custom all the year, except that each is allowed, in his turn, an absence of about a month, which he spends

with

with his family; and it is remarkable, that the families of these shepherds reside entirely in Leon.

The shepherds who came with his majesty's flock were questioned on the subject of giving salt to their sheep: they declared that this is only done in the hottest season of the year, when the sheep are on the mountains; that in September it is left off; and that they dare not give salt to ewes forward with lamb, being of opinion that it causes abortion.

It is scarcely credible, though it appears on the best authority to be true, that under the operation of the laws of the Mesta, which confide the care of the sheep to the management of their shepherds, without admitting any interference on the part of the proprietor, no profit of the flock comes to the hands of the owner, except what is derived from the wool; the carcases of the culled sheep are consumed by the shepherds*, and it does not appear that any account is rendered by them to their employers, of the value of the skins, the tallow, &c.; the profit derived by a proprietor from a flock, is estimated on an average at about one shilling a head, and the produce of a capital vested in a flock is said to fluctuate between five and ten per cent.

The sheep are always low kept. It is the business of each Mayoral to increase his flock to as large a number as the land allotted to it can possibly maintain: when it has arrived at that pitch, all further increase is useless, as there is no sale for these sheep, unless some neighbouring flock has been reduced by mortality below its proper number: the most of the lambs are therefore every year killed as soon as they are yeaned, and each of those preserved is made to suck two or three ewes; the shepherds say, that the wool of an ewe that brings up her lamb without assistance is reduced in its value.

At shearing time the shepherds, shearers, washers, and a multitude of unnecessary attendants, are fed upon the flesh of the culled sheep; and it seems that the consumption occasioned by this season of feasting is sufficient to devour the whole of the sheep that are draughted from the flock. Mutton in Spain is not a favourite food; in truth, it is not in that country prepared for the palate as it is in this. We have our lamb-fairs, our hog-fairs, our shearling-fairs, our fairs for culls, and our markets for fat sheep; where the mutton, having passed through these different stages of preparation, each under the care of men whose soil and whose skill are best suited to the part they have been taught by their interest to assign to themselves, is offered for sale; and if fat and good, it seldom fails to command a price by the pound, from five to ten per cent. dearer than that of beef. In Spain they have no such sheep-fairs calculated to subdivide the education of each animal, by making it pass through many hands, as works of art do in a manufacturing concern; and they have not any fat sheep markets that at all resemble ours. The low state of grazing in Spain ought not therefore to be wondered at, nor the poverty of the Spanish farmers; they till a soil sufficiently productive by nature, but are robbed of the reward due to the occupier, by the want of an advantageous market for their produce, and the benefit of an extensive consumption; till the manufacturing and mercantile parts of a community become opulent enough to pay liberal prices, the agricultural part of it cannot grow rich by selling.

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* The shepherds, on discovering the drift of the questions put to them on this head, said that in settling the wages of the shearers and washers, at the Esquileos, allowance is made for the mutton with which they are fed.

That the sole purpose of the journeys taken annually by these sheep is to seek food in places where it can be found; and that these migrations would not be undertaken, if either in the northern or the southern provinces a sufficiency of good pasture could be obtained during the whole year,—appears a matter of certainty. That change of pasture has no effect upon their wool, is clear, from all the experiments tried in other countries, and in Spain also: for Burgoyne tells us, that there are stationary flocks, both in Leon and in Estremadura, which produce wool quite as fine as that of the Trashumantes.

The sheep lately presented to his majesty are of the Cavaña of Paular, one of the very finest in point of pile, and esteemed also above all others for the beauty of carcase. In both these opinions, M. Lasteyrie, a French writer on sheep, who lived many years in Spain, and paid diligent attention to the Merino sheep, entirely agrees: he also tells us, that the Cavaña of Negrete, from whence the sheep imported by his majesty in the year 1791 were selected, is not only one of the finest piles, but produces also the largest-carcased sheep of all the Merinos. Mr. Burgoyne agrees with him in asserting, that the piles of Paular, Negrete, and Escurial, have been withheld from exportation, and retained for the royal manufactory of Gaudalaxara, ever since it was first established.

The Cavaña of Paular consists of 36,000 sheep. It originally belonged to the rich Carthusian monastery of that name, near Segovia; soon after the Prince of the peace rose into power, he purchased the flock from the monks, with the land belonging to it, both in Estremadura and in Leon, at a price equal to twenty French franks a head, 16*s*. 8*d*. English. All the sheep lately arrived are marked with a large M. the mark of don Manuel.

The number sent from Spain to the king was 2000, equal to two subdivisions of the original Cavaña. To make the present the more valuable, these were selected by the shepherds from eight subdivisions, in order to choose young, well-shaped, and fine-woolled animals. This fact is evident, from the marks which are placed on eight different parts of the bodies of the sheep now at Kew.

The whole number embarked was 2,214; of these, 214 were presented by the Spaniards to some of his majesty's ministers, and 427 died on the journey, either at sea or on their way from Portsmouth to Kew. His majesty was graciously pleased to take upon himself the whole of the loss, which reduced the royal flock to 1573; several more have since died. As the time of giving the ram in Spain is July, the ewes were full of lamb when they embarked, several of them cast their lambs when the weather was bad at sea, and are rendered so weak and infirm by abortion, that it is much to be feared more will die, notwithstanding the great care taken of them by his majesty's shepherds. A few have died of the rot. This disease must have been contracted by halting on some swampy district, in their journey from the mountains to the sea at Gijon, where they were embarked, as one sheep died rotten at Portsmouth; there is every reason however to hope, that the disease will not spread, as the land on which they are now kept has never been subject to its ravages, being of a very light and sandy texture.

It is well worthy of observation, that although the Swedes, the Saxons, the Danes, the Prussians, the Austrians, and of late the French, have, either by the foresight of their governments, or the patriotic exertions of individuals, imported Merino sheep, no nation has hitherto ventured to assert, that they possess the complete and unmixed race of any one Cavaña; this circumstance does not appear to have been attended to any where but in England; though in fact, each Cavaña is a separate and distinct breed of sheep, not suffered by the Spaniards to mingle with others. The difference in value of the wool of different Spanish flocks is very great; at this time, when Spanish wool is unusually dear, the prima piles are worth more than *7s.* a pound, and yet the inferior ones scarce reach *5s.** Even the French, attentive as that nation is to all things that concern the interest of individuals, appear to have overlooked this circumstance, and to have contented themselves with making up the numbers of their importations, without paying any regard to it; they have not at least stated in any of their publications, that attention was paid to the securing sheep of a prima pile, and keeping the breed of that pile pure and unmixed after they had obtained it.

Our merchants in Spanish wool range the prima piles in the following order of value, as appears by a statement in the year 1792.

Paular.
Negrete.
Muro.
Patrimonio; and 15 more not necessary to be enumerated.

M. Lasteyrie, the French writer on sheep, ranges them not very differently; he states them as follows: but both English and French agree that all the prima piles are nearly equal in fineness of fibre, and consequently in value to the manufacturer.

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* Since this was written, Spanish wools have risen to an exorbitant price. Prima Leonesa is this week rated in the Farmer's Journal at *20s.* a pound, and Seville at *13s. 6d.*

Escurial, called by us Patrimonio,
Guadalupe,
Paular.
Infantado.
Montareo.
Negrete, &c.

The Danes, he tells us, procured their sheep from the best piles; but there is no appearance of their having, since they obtained them, kept the flocks separate, nor are they at present so remarkable for fine wool as the Saxons, whose wool is now at least as fine as that of Spain is, upon an average of prima and second rate piles.

The Swedes were the first people who imported the Spanish breed. This good work was undertaken and completed by the patriotic exertions of a merchant of the name of Alstroemer, in the year 1723, The next who obtained an importation of Merino sheep were the Saxons, who are indebted for the benefits they enjoy from the improvement of their wools to the prince Xavier, administrator of the electorate during the minority of the elector, and brother-in-law to the king of Spain. The prince obtained a flock of these valuable animals in 1766, and in 1778 an addition to it of 100 rams and 200 ewes. The Danes followed his useful example, as also did both Prussia and Austria. Every one of these countries continue at this moment to profit largely by the improvement these sheep have occasioned in their agricultural concerns. So far from truth is the too common assertion, that their wool will not continue fine in any country but Spain, that in the year 1806, when the ports of Spain were closed against us, a very large quantity of fine wool, the produce of German Merino sheep, was imported into this country from Hamburgh, and used by our manufacturers as a substitute for Spanish wool. In truth, some of this wool was so fine that it carried in the British market as high a price as the best Spanish piles were sold for, in times of peace and amity.

[To be continued.]

XLII. *On*

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XLII. *On the Motion of Floating Bodies.*

March 4, 1809.

SIR,

In your Magazine of the last month, Capt. Burney made certain experiments on unloaded and loaded barges, with respect to their velocities in a running stream: and there was an allusion also to beams, or sticks, or timber, always moving with the heavy end foremost. These two problems may be solved in the same way,—they are the result of gravity. The heavier a barge is loaded the quicker will it move, because all water that is in motion moves down an inclined plane seeking its level; the loaded barge that swims or floats in it will of course move down a regular inclined plane, endeavouring by the force of gravity to descend, and its velocity will be in proportion to its weight; viz. to the quantity of matter moving together; and unless resisted or opposed by a contrary force, or irregular currents, it will acquire an increased velocity in a certain ratio. This is exactly a parallel case to loaded or unloaded carriages going down hill, the heavier they are, with the greater velocity will they press downwards;—or, to put another case, let a cannon shot and a round piece of turned wood of the same dimensions be rolled down an inclined plane, the cannon ball will roll quickest, because it contains more matter; in the same way it would descend quicker through the air.—A beam or stick in the water observes exactly the same laws of matter and motion, and will go down the stream with its heavier end foremost: so in the air, if a stick be thrown upwards, the heavier end will first reach the ground.—The savages in the South Sea Islands know this, and make certain short spears or clubs, which they throw at their enemies, over wherever they see a crowd of them; and these clubs fall with the heavy end downmost, and if they hit disable or kill. I am, sir, your obedient servant,

G. Orr.

To Mr. Tilloch.

XLIII. *Pro-*

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XLIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

March 2.—The reading of Mr. Home's paper on the intervertible joint discovered in the basking shark, was concluded. Mr. Brandé analysed the liquor found in this peculiar joint, when it proved to be almost entirely animal mucus or mucilage, without either gluten or albumen.

March 9—16.—Earl of Morton, vice-president, in the chair. A very long memoir was read on the nature and modifications of coloured concentric rings, exhibited in glasses brought into contact, by Dr. Herschel. It is impossible to give any adequate idea of the numerous and diversified experiments performed by this indefatigable philosopher, whose narrative of them is divided into above 60 subdivisions. From these experiments it appeared that no coloured rings were produced if the glasses were of the same quality, uniformly level, and brought perfectly into contact. Sir Isaac Newton's opinion respecting "fits of transmission" was explained on his own principles of the known difference of refrangibility of coloured rays. Various other optical phænomena, relating to coloured rays, chromatics, and refraction, were incidentally illustrated.

March 22.—Earl of Morton in the chair. An account of experiments on Brazilian platina, by Dr. Wollaston, was read. The very small specimen of platina from the silver mines in Brazil, which Dr. W. analysed, was given to him by the chevalier de Souza, the Portuguese minister in this country. Vauquelin having found platina, but no palladium, in the silver mines of Guadalcanal, it was thence supposed that this metal was peculiar to the Peruvian platina. The Brazilian platina, however, has some external characters different from that of Peru; it is brighter, flat, not rounded off at the corners, and has not that *worn* aspect which the Peruvian platina presents. It also contains a small quantity of gold, which was not found in the platina of Estremadura. But notwithstanding the smallness of the specimen, native palladium was discernible in it by its external characters: although white, like the platina, it exhibited flat square surfaces, which were laminous, and could be mechanically detached from the other metals. The specimen which Dr. W. received was too small to admit of his ascertaining the exact proportions of native palladium, gold, and platina it contained; but on examining the palladium apart, and dissolving it, some sensible traces of iridium were discovered; and the Doctor supposes that, when sufficiently large specimens of the Brazilian platina are received, it will be found to contain not only palladium and gold, but also iridium and osmium, like the Peruvian platina.

WERNERIAN NATURAL HISTORY SOCIETY.

At the meeting of this Society on the 11th of February, Professor Jameson read a short account of the oryctognostic characters and geognostic relations of the mineral named cryolite, from West Greenland.

Mr. P. Neill read a description of a rare species of whale stranded near Alloa, in the Frith of Forth, in the end of October last. It was 43 feet long; had a small dorsal fin very low down the back; longitudinal folds in the skin of the thorax; short whalebone (*fanons*) in the upper jaw; the under jaw somewhat wider, and a very little longer than the upper; both jaws rather acuminated, the under one ending in a sharp point proceeding from a twisted bony ridge on the lower side. From these characters he considered it as evident that it was the Baleinoptera acuto-rostrata of La Cepede, and that that author had fallen into an error in saying that this species never exceeds from 26 to 29 feet in length.

At the same meeting, the Secretary laid before the Society several interesting communications. 1. Copies of the affidavits made before the justices of the peace at Kirkwall, in Orkney, by several persons who saw and examined the carcase of the great sea snake (Halsydrus Pontoppidani) cast ashore in Stronsa in October last; with remarks illustrative of the meaning of some passages in these affidavits.—2. An account of the discovery of a living animal resembling a toad, inclosed in a bed of clay, (in a cavity suited to its size, and which retained its shape,) at the depth of fifty-seven fathoms, in the coal-formation at Govan; communicated by Mr. Dixon of Govan-hill.—3. An instance of remarkable intrepidity displayed by an old male and female otter, (at the river Dart, near Totness, Devonshire,) in defending their young, although the Otter is generally accounted a very timid animal; communicated by Mr. Laskey of Crediton.

At this meeting also, Mr. Laskey (who is at present with his regiment in Scotland, and who is well known in the scientific world as an eminent conchologist,) presented to the Society a very valuable and well arranged collection of British shells, and likewise a curious mineral from New Holland.

At the meeting of this Society on the 11th of March, Dr. Yule read an interesting memoir on the natural order Gramineæ, with introductory observations on monocotyledonous plants, in which he contrasted these with the dicotyledonous class, from the period of germination to the complete evolution of their stems. The Doctor is to continue the subject in a future paper.

Capt. Laskey laid before the Society a list of Scottish Testacea, as far as they had fallen under his own observation; with remarks on the new and rare species. Of the genus Chiton he enumerated 4 species; of Lepas 3; Balanus 6; Pholas 4; Mya 9, including 3 new species; of Ligula, (a lately constituted genus,) 7 species; Solen 6; Tellina 15, including a new species, named by Col. Montagu, *T. Laskeyi;* Cardium 10; Mactra 6; Donax 3; Venus 23, including 9 new species; Chama 1; Arca 6; Pecten 6; Ostrea 1; Anomia 4; Mytilus 11; Pinna 1; Nautilus 3; Cypræa 1; Bulla 13, including 2 new species; Voluta 8, 4 of them new; Buccinum 8; Strombus 2; Murex 23, comprehending the rare carinatus, and 3 new ones; Trochus 4; Turbo 32, 5 new; Helix 17; Nerita 7; Haliotis 1; Patella 11; Dentalium 2; Serpula 7; Vermiculum 3. This is the most ample catalogue of Scottish testacea hitherto formed; containing 126 species of multivalve and bivalve, and 142 species of univalve shells; in all 268.

At the same meeting the Secretary read a communication from George Montagu, esq., of Knowel House, giving an account of a nondescript fish, five feet long, taken on the coast of Devonshire last summer. It must constitute a new genus, in the Apodal order; and Mr. Montagu has bestowed on it the generic name of *Ziphotheca*, and the specific one *tetradens*. The communication likewise contained accurate descriptions of four rare species of English fishes; and was accompanied with correct and elegant drawings of these, as well as of the ziphotheca.—At the same time, Mr. Montagu presented the Society with copies of his Testacea Britannica, and Supplement, three vols. 4to., with coloured plates, and of his Ornithological Dictionary, two vols. 8vo.

XLIV. *List of Patents for New Inventions.*

To John Dickinson, of Ludgate Hill, London, stationer, for certain improvements on his patent machinery for cutting and placing paper; and also certain machinery for the manufacture of paper by a new method. Jan. 19, 1809.

To George Finch, jun., of King Street, Soho, orris weaver, for certain methods of manufacturing various kinds of metal laces, so as to imitate gold and silver laces; and also of manufacturing gold and silver open laces. Feb. 4.

To Thomas Potts, of Hackney, for a new process of freeing tarred ropes from the tar, and rendering them fit for the use of the manufacturer. Feb. 4.

To Frederick Albert Winsor, of Pall Mall, esq., for certain improvements upon his former patent oven stove, or apparatus for carbonising all sorts of raw fuel and combustibles, and reducing them into superior fuel of coke and charcoal, as well as for extracting and saving, during the same process, the oil, tar, pyroligneous vegetable acid and ammoniacal coal liquors; and for extracting and refining all the inflammable air or gas, so as to deprive it of all disagreeable odour during combustion, and rendering the gas itself salutary for human respiration, when properly diluted with atmospheric air. Feb. 7.

To William Congreve, of Cecil Street, Strand, esq., for his mode of construction or arrangement for any building, so as to afford security against fire, with other advantages. Feb. 7.

To Archibald Thomson, of Manchester, engineer, for certain improvements on machines applicable to various kinds of spinning. Feb. 7.

To William Everhard Baron Doornik, of Old Lisle Street, Leicester Square, for certain improvements in the manufacture of soap, to wash with sea-water, with hard-water, and with soft water. Feb. 7.

To John Stead, card-manufacturer, Leith Walk, Edinburgh, for his method of manufacturing cards which are employed in the carding and spinning of flax, tow, wool, cotton, and silk, so as to combine the quality of a fine card with the strength of a coarse one. Feb. 9.

To James Grellier, of Aldborough Hatch, in the county of Essex, esq., for a peculiar construction for the purpose of burning coke and lime, whereby the superfluous heat of the fire used in burning the coke is applied to burn the lime, and also whereby such fire may be rendered perpetual, and which he denominates "the union and perpetual kiln." Feb. 13.

To Stephen Hooper, of Walworth, in the county of Surrey, gent., for a thermometer or machine for ascertaining the heat of bakers' ovens and various other purposes. Feb. 13.

To David Meade Randolph, a citizen of Virginia, in the United States of America, but at present residing near Golden Square, in the county of Middlesex, merchant, who, in consequence of a communication made to him from his friend and correspondent residing within the said United States, has become possessed of a new method of manufacturing all kinds of boots, shoes, and other articles, by means of a substitute for thread made of hemp, flax, or other yarns. Feb. 21.

To Joseph Ilett, of Stratford, in the county of Essex, calico-printer, for his method of producing fast greens on cotton and various other articles. Feb. 21.

To Leger Didot, of Two Waters, in the county of Hertford, for certain improvements in the construction of umbrellas and parasols. March 1.

To Richard Scantlebury, of Redruth, in the county of Cornwall, brazier, for a machine, by which he counterbalances the weight of any volume of water or other fluids, required to be lifted by any steam or water engine, or other machinery, either worked by animals or men, which gains a very considerable power over any machine now in use. March 1.

To Edward Steers, of the Inner Temple, esq., for a new method directed by machinery, of using the screw, by which its mechanical power or its motion is increased. March 1.

To Abraham Seward, of Lancaster, tin-plate worker, for a new improved hook, for bearing up the heads of horses in drawing carriages. March 1.

To Thomas Clatworthy, of Winsford, in the county of Somerset, sheep shears maker, and John Clatworthy, of the same place, sheep shears maker, his son, for shears on an improved construction for shearing sheep. March 1.

To Frederick Bartholomew Folch, of Oxford Street, and William Howard, of Bedford Street, Lockfields, in the county of Surrey, for a certain machine instrument or pen, calculated to promote facility in writing; and also a certain black writing ink or composition, the durability whereof is not to be affected by time, or change of climate. March 4.

To William Proctor, of Sheffield, optician, for improved methods of raising or supplying tubes or lamps with oil, so as to remove away the shade of the vessel containing the oil, and in form and use equal to any mould or wax candle, which he denominates Proctor's spiral Argand and candle lamp. March 9.

To John Heathcoat, of Loughborough, in the county of Leicester, lace manufacturer, for a machine for the making and manufacturing of bobbin lace, or lace nearly resembling foreign lace. March 20.

To James Hakewill, of Beaumont Street, in the parish of St. Mary-le-bone, artist, for an improvement in the construction of tables, chairs, and stools, for domestic, military, and naval service, and in the packing of the same. March 20.

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For March 1809.

Days of the Mouth.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.			
Feb. 25	40°	47°	42°	30·26	36	Fair
26	33	47	43	·42	35	Fair
27	40	51	40	·35	37	Fair
28	33	53	46	·30	36	Fair
March 1	46	47	40	·25	0	Rain
2	40	45	34	·40	25	Cloudy
3	33	44	42	·33	37	Fair
4	41	43	40	·16	20	Showery
5	39	42	35	·18	7	Small rain
6	33	39	33	·29	9	Cloudy
7	33	39	35	·38	5	Cloudy
8	35	43	39	·47	15	Fair
9	33	54	50	·25	29	Cloudy
10	42	48	36	·24	30	Fair
11	33	45	40	·19	33	Fair
12	40	48	36	·09	57	Fair
13	33	45	35	·20	51	Fair
14	41	45	36	·26	35	Fair
15	36	44	41	·40	35	Fair
16	37	51	45	·17	41	Cloudy
17	42	53	42	·18	35	Cloudy
18	39	53	41	·05	39	Fair
19	40	51	44	29·99	32	Cloudy
20	41	49	41	30·00	30	Cloudy
21	40	48	44	·14	15	Cloudy
22	44	58	44	·02	52	Fair
23	44	59	51	29·88	64	Fair
24	47	56	48	·62	55	Fair
25	44	49	42	·30	54	Fair
26	44	53	40	·17	35	Cloudy

N. B. The Barometer's height is taken at one o'clock.

XLV. *Observations on a late Paper by Dr.* Wm. Richardson, *respecting the basaltic District in the North of Ireland, and on the Geological Facts thence deducible; in Conjunction with others observable in Derbyshire and other English Counties: with the Application of these Facts to the Explanation of some of the most difficult Points in the Natural History of the Globe. By Mr.* John Farey.

> "By making ourselves acquainted with *effects*, we shall be better qualified to investigate *causes*; and if those effects shall appear to be beyond the powers of such natural agents as we are already acquainted with, we shall be justified in admitting the performance of operations to which we have seen nothing similar; and also in admitting the former existence of powers of far superior energy to any we have ever known in action."—Dr. W. Richardson.

To Mr. Tilloch,—Sir,

Next to the delight occasioned by the discovery of any truth of important application, few things can be more pleasant to an ingenuous mind, than to observe others arriving at a similar conclusion, by modes sufficiently distinct to give additional evidence to the truth acquired.

I was led to these reflections, from having considered all that I had read or heard, concerning the *basaltic districts* of our globe, previous to the perusal of Dr. *William Richardson's* late able paper in the Philosophical Transactions, (reprinted in your two last Numbers, and which I shall take the liberty therefore of referring to,) as showing, that no part of the surface or crust of the whole earth was *less likely* to harmonize with the conclusions, to which I had been led, seven or eight years ago, by an attentive consideration of the geological facts which Bedfordshire then presented to my mind, and which have since received ample, and I think I might say complete confirmation, in a more extended field of observation.

Of the nine geological facts deduced by Dr. Richardson, as applicable to his basaltic district or area, seven of them appear exactly conformable to all my experience in other districts, including very various kinds of strata; and perhaps my not fully comprehending some expressions in his 4th and 5th facts, (page 113,) may alone have prevented a like concurrence as to them. It gives me great pleasure therefore to find, the results of my observations on the *denudated* districts in Sussex, Derbyshire, &c., communicated fully to numerous friends within three years past, which are shortly alluded to in your volumes (xxviii. p. 120; and xxxi. p. 37), and more fully explained under the article *Denudation*, and others, in *Dr. Rees's New Cyclopædia*, thus fully confirmed by Dr. Richardson's able investigations, conducted, as far as I am acquainted, without any knowledge of what I have been doing, and tending to remove all doubts as to the regular stratification of basalt.

The *cutting* and *carrying off* of the upper strata, observable in numerous instances in the north of Ireland, has been termed by Dr. R. (pages 114, 196,) ABRUPTIONS of the strata, which word I am not inclined to adopt instead of DENUDATIONS already explained, as above. The word HUMMOCK, introduced by Dr. R., notwithstanding the seamen have limited its use to circular knowls or points of hills, may have its meaning as a geological term extended, as Dr. R. has done, to include such as are precipitous and irregular also in their shape, and as such I shall hereafter adopt it, instead of *cap*, (a term much too numerous in its meanings already,) which I have hitherto used, in pointing out to my friends in Derbyshire, the numerous *hummocks* on their denudated hills; where these detached pieces of strata, being mostly accessible on all sides, have furnished the strongest evidence both to myself and others on the spot, (as similar ones have done in Ireland to Dr. Richardson,) that the intervening parts of the same stratum, once continuous, have been torn off from our globe.

Before I had ever seen a hummock or heard of a denudated district, from observing the universality of fissures or *faults* in Bedfordshire, having their sides always *polished* or *worn*, pursuing rectilinear courses, quite inconsistent with the crater-like action of any force hitherto supposed to have acted *from below*, I was irresistibly led to the consideration of forces *acting from above*, as Dr. R. has also been, by the evident *excavation* of valleys and leaving of *hummocks* in his basaltic area.

The worn state of those *faults*, which make at present no alteration in the level of the strata on each side of them, in common with those which, as doing otherwise, might admit of explanation by the mere *slip* or subsidence of one side, obtruded the conclusion, that *successive* and *general heavings of the surface* were necessary, to account for this phænomenon, so universally overlooked by geologists in their writings; and *Gravity*, that most powerful of known agents, which, now that no satellitic body remains nearer to the earth than 240,000 miles, daily heaves up a mass or column of sea water, perhaps 1000 miles diameter and ten feet high! appeared to me as the probable cause, through the medium of a large and perhaps very dense body, that might have revolved round this globe, at that awful but important period, when "God said, Let the waters be gathered together, and let *the dry land* appear."

These ideas of accounting for the *universality and worn state of faults*, I had very shortly after the opportunity of explaining to the worthy President of the Royal Society, when on a visit to that inestimable character the *late Duke of Bedford*, in a day's ride over the district which had furnished the materials for these speculations, and while the progress of His Grace's extensive works then carrying on, admitted of verifying most of the facts. The sudden loss of my former patron, having occasioned the turning of my attention more particularly to the acquirement of geological knowledge, I have since had the happiness of finding these first ideas of mine, when applied to a *satellite* moving near enough and with attraction sufficient, to *reverse the direction of gravity* for the instant of its passage, over any given tract on the earth's surface, as fully adequate to account for the numerous, and to me new and astonishing facts, which my researches in Sussex, Derbyshire, Staffordshire, &c., have since furnished: the details of these I intend to publish, as soon as my observations on Derbyshire and the surrounding borders of other counties shall be completed, and my professional avocations will allow. In the mean time, I have been anxious, to suggest the above effects and their causes, for the consideration of those, who, like Mr. *Wm.*

Smith, Dr. Richardson, M. Andre, &c., may engage in extended and minute inquiries, as to "*the actual surface of the earth,*" (p. 170 of the present volume,) without which direction to their inquiries, the mention of *geology* must continue to be received with a smile, as *M. Cuvier* and his very able associates justly remark.

In order to show that *hummocks* or isolated caps of strata are not confined to basalt, or any other stratum in particular, but are of common occurrence in denudated districts, I beg to present the following list of a few which I have observed, and noted most of them, in the part of my geological map of Derbyshire, a copy of which has been now some time in possession of my worthy patron in these pursuits, the President of the Royal Society, referring for some further particulars, to my Section plate II. of your thirty-first volume.

Hummocks with *Coal measures* on their tops.
    Hill North of Ounston, near Dronfield,
    Shutlings Low, near Macclesfield, Cheshire.

Hummocks with *First* or *Millstone Grit* on them.
    Stanton Moor, near Winster,
    Hartle Moor, ditto,
    Comb's Moss, near Buxton,
    Lose Hill, N.E. of Hope,
    Grindlow Rime, N. of Edale,
    Sheenhill, near Longnor, Staffordshire,
    Revedge, near Warslow, ditto.

Sometimes these appear, as single or romantic isolated *Rocks* on the millstone grit districts, as
    Alport Tor-stone, in Wirksworth,
    Thoma's Chair, on Stanton Moor, near Winster,
    Endle Stone, - ditto, - ditto,
    Rowter Rocks, at Birchover, - ditto,
    Mock-beggar Hall, on Hartlemoor, ditto.

Hummocks or Caps of part of the *First Limestone.*
    Gree Tor, S. of Winster,
    Bank's Pasture Rocks, ditto,
    Dungeon Rocks, near Wensley,
    St. Peter's Rocks, in Cresbrook vale, near Wardlow.

Hummocks

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Hummocks of the *Second Limestone.*

Hobthurst Houses, in Wey-dale, near Little Longstone,
Hill N. of Miller's dale, near Tidswell,
Wormhill, near ditto,
Tunstead Hill, near Wormhill,
Bole-end Hill, ditto.

Hummocks of the *Third Limestone.*

Aldwark, near Brassington,
Hill N.E. of Green Fairfield, near Buxton,
Buxton Town.

Hummocks of the *Third Toadstone.*

Staden Hill, near Buxton,
Knot Low, near Wormhill,
Cawton Low, near Chelmerton,
Harborough Rocks, near Brassington.

The hummocks of the *Fourth Limestone* differ from many of the others above, in our not being able to see these isolated masses, resting on any under stratum, since none of the very deep valleys which intersect it, are excavated deep enough, to reach any under measures. The numerous isolated conical and peaky hills in Hartington, and other parishes on the W. side of the Limestone district, are all a sort of massive hummocks, too numerous here to be named.

In Dove-dale, in this stratum very extraordinary small hummocks occur, or rather perhaps, in the rude and very wide barren *veins* by which the vale was intersected :—these are called Tissington Spires, the Sugar Loaves, &c.—Reynard's Tor, Hoc-cliff, Pike Tor, &c., in Brassington, are also among the interesting hummocks of this stratum.

The above list, contains none of the many curious and conspicuous hills of this district, which are crowned by projecting or suddenly elevated points or edges of strata, still connected with the mass, but only such whose strata or upper beds, are entirely isolated by a surrounding denudation.

Small hummocks of *Gravel* on the heights of this denudated tract, consisting of sand, mixed with quartz and other very hard and highly rounded pebbles, (not of the rocks of any known part of the globe, as has been said,) at some miles distance from any other gravel, are perhaps among its most curious phænomena:—these I have observed at

Thorney Ley, W. of Chapel le Frith,
E. of Kilburn, near Horsley,
S. of Strelly, Notts,
On Sheepston Hill, W. of Annesley, Notts.

After revolving the circumstances of *excavated valleys* in my mind, as I have travelled in these pursuits, for weeks and months together, and observed these valleys wonderfully distributed over the whole surface of large districts, effecting a descending outlet or drainage to *every part thereof*, as perfectly, though with none of the constant regularity, in which the veins are distributed over an animal, for returning its blood from every part to the heart: I have been lost in conjecturing any application of mechanical or known principles, that could have directed the almost irresistible forces which effected this important, and as it were, finishing operation upon the matters of our globe, but must refer the same to Omnipotent Power itself, acting perhaps in this instance, without the intervention of the agents, whose operations in nature the light of science enables us in so many instances to trace.

Dr. Richardson's expressions in his 3d fact, (p. 112) and mine above, might perhaps be construed as asserting, that the form of the surface of a denudated tract or excavated valley, is uninfluenced by the arrangement or alternations of its strata: such, however, is seldom strictly the case; for though to a cursory or inexperienced observer, the contour of most valleys and hills seems regular, except where there are façades or cliffs, yet a more attentive examination of the outline of such denudated or abraded surfaces, will discover the edge or top of every stratum which is materially harder than that above it, as grit-stone under clay, &c., by means of a slight protuberance, or tablet as some have called it, visibly projecting the surface, but so slightly as in most instances to have escaped the notice of persons on the spot, and yet I have scarcely ever failed of late, in being able to discover the position of the strata in a denudated tract, composed of several strata, by this means alone, and frequently at some distance, and can often distinctly trace the bases to three or four different strata at the same time, as I ride along, sometimes for miles together, before any pits or quarries are found open, by which to identify the substances of which they severally consist.

I mention the above, both as a circumstance of great practical importance in mineralogical surveying, and also as tending to prove, that the strata had acquired their present comparative hardness, before the denudation and excavations spoken of, took effect: the faults likewise have happened since the consolidation of the strata, as their ground edges in numerous instances prove, and the disarrangement they occasioned in the strata must likewise have occurred, prior to the final denudations and excavations of the surface, since very few of the numerous faults which raise the measures on one side or depress those on the other, are visible by any inequalities on the surface, except that a very attentive and experienced eye may often discover their situation, by means, of the interruptions they give to the faint tablets of strata, above described: by which, I have sometimes greatly surprised practical miners in tracing out the principal faults of their district: a circumstance often of the utmost importance in practical mining.

I cannot conclude this letter, without heartily congratulating Dr. Richardson on the very great progress which he has made in these inquiries, and expressing a hope, that he will still persevere; endeavouring also to bring fresh labourers into the field, for the purpose of giving us a general and connected idea, of the order and position of all the principal strata of the interesting island wherein he resides. Maps also, showing by different colours, the surface occupied by each particular stratum, and vertical sections in particular directions similarly coloured, are much wanted, and will, I hope, ere long, be undertaken.

I am, sir, your obedient servant,

JOHN FAREY, Mineralogical Surveyor.

12, Upper Crown Street, Westminster,
April 5, 1809.

XLVI. *Ana-*

XLVI. *Analysis of the Mécanique Céleste of M.* La Place, *Member of the French Institute, &c. By M.* Biot *.

Newton, by publishing his Principia and the immortal discovery of universal gravity, gave a new direction to the physical and mathematical sciences. He was the first who demonstrated that, in order to discover truth in the study of nature, it was not necessary to imagine precarious causes in order to deduce from them hypothetical results, but to ascend by a course of well-directed inductions from the phænomena observed to the laws which produce them; and in this point of view we may regard this great man as having prepared the way for all the discoveries of his successors. Newton presented under the synthetical form, results which might probably have been attained by a different route; and herein he perhaps attached himself to his avowed predilection for the method of the ancients; and probably, also, he gave way to a desire of concealing the course which he had pursued. Modern geometricians, without entirely abandoning constructions, which are always satisfactory to the mind, have felt that the assistance of analysis was necessary for giving to the principle of universal gravity all the developments of which it is susceptible; and it is to this happy idea, and to the progress of the integral calculus, that the theory of the system of the world owes the perfection which has now been attained; a perfection so great, that there does not exist any astronomical phænomenon, the causes and laws of which cannot be assigned. But these valuable discoveries, the results of the labours of a small number of men, were too isolated from each other, and the chain by which they were united too difficult to unravel, in order to bring them within the reach of the greater number. It became important therefore to collect them in a work of the same nature, but in a form different and more complete than that of Newton. This task required an equally intimate acquaintance with astronomy and with analysis, and particularly that philosophical mind which discusses phænomena

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* Translated from the French.

nomena with care, compares them with each other, and, removing the illusions of imagination and of the senses, penetrates to the true laws of nature. In these respects the task fitted M. La Place exactly, who from the outset of his career directed his researches towards the celestial phænomena, and who has since taken an active part in the progress of this science, by publishing, upon every point connected with the system of the world, a crowd of Memoirs filled with important discoveries. It is principally from these memoirs that M. La Place has derived the materials of this great work: and if he has connected them with each other by an admirable coincidence, it has arisen from all of them having become peculiar to himself, either because he had been the first to discover them, or from the new form which he has given to them.

Astronomy, considered under the most general point of view, is a great problem in mechanics, the elements of which are furnished by observations. This problem is very susceptible of being submitted to calculation; because the immense distances which separate the celestial bodies, attenuating the secondary causes, which might act upon them, in order to bring into view only the principal forces which animate them, give to their movements a rigour and precision truly mathematical. To develop the relations which exist between the motions and forces which produce them; to deduce from thence the nature of the force which ought to animate celestial bodies, in order that their movements may be such as are presented to us by observation; thus to raise ourselves to the principle of universal gravity, and to re-descend from this principle to the explanation of all the celestial phænomena, even to their minutest details, such is the object of the *Mécanique Céleste,* and such has been the object of the labours of M. La Place.

BOOK FIRST.

After having first detailed the principles of the composition and decomposition of forces, the author establishes the conditions of equilibrium for any point wanted, by any number of forces acting in any given directions; conditions which reduce it to this, namely, The sum of the products of each force by the element of its direction is null. He teaches us to determine, when the point is not free, the pressure exercised by it upon the surface or upon the curve to which it is subjected. Considering afterwards the point in the state of motion, he seeks the relation which exists between the forces that animate it and the velocities which should result from it; and by a very delicate analysis, and considerations drawn from experience, he demonstrates that, in nature, this relation of the force to the velocity is the proportionality. After having developed the immediate consequences of this law, the author gives the equation of the movement of a point animated by any given forces, and determines the pressures exercised by this point upon the surface or upon the curve to which it may be subjected. He afterwards makes the application of these principles to the motion of bodies animated by gravity in a resisting medium, and to that of a point gravitating upon a spherical surface. The isochronism of the very small oscillations of this moveable point leads to the problem of *tautochrones* which the author resolves, in the case where the resistance of the medium is proportional to the two first powers of the velocity. He is afterwards occupied with the conditions of the equilibrium of any system of bodies considered as points: he writes down for each of them the equation of the equilibrium; and uniting these results, he extracts from it the principle of the virtual velocities, which is thus demonstrated in a direct and general manner. After having shown how we deduce from this the reciprocal actions of the bodies of the system, and the pressures which they exercise upon external obstacles, he makes the application of them to the case in which all the points of the system are invariably united together; and this leads him to treat of the centre of gravity. The author afterwards considers the conditions of the equilibrium of fluids: the property which characterizes them being a perfect mobility, it is necessary, in order that a fluid mass be in equilibrium, that each of the molecules composing it be in equilibrium in virtue of the forces which animate it. The author, setting out from this principle, determines the rela-

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tion which should exist between the forces which solicit the system in order to fulfil this condition, and he makes application of it to the equilibrium of a homogeneous fluid mass covering a fixed solid nucleus, and of a given figure. He afterwards gives the general equation of the movement of any system of bodies, which he deduces from that of equilibrium; and he draws from it the principles of the preservation of living forces, of areas, of the motion of the centre of gravity, and of the least action. He fixes the circumstances in which these principles take effect, and gives the method of estimating the alteration which that of living forces undergoes in the sudden changes of the motion of the system. In treating of the principle of the areas, he shows that in the motion of a system of bodies animated solely by their mutual attraction, and by forces directed towards the origin of the coordinates, there exists a plane passing by this origin, and which enjoys the following remarkable properties: 1st, The sum of the areas traced upon this plane by the projections of the *vector radii* of the bodies, and multiplied respectively by their masses, is here the greatest possible. 2dly, This same sum is null upon all the planes which are perpendicular to it; the principles of its living forces and of the areas, still taking place with respect to the centre of gravity, even supposing it to have an uniform and rectilinear movement. Hence it results that we may determine a plane passing by this moveable origin, and upon which the sum of the areas described by the projections of the *vector radii* of bodies, and multiplied respectively by their masses, is the greatest possible. The author shows that this plane is parallel to that which passes by the fixed origin, and satisfies the same conditions. Hence he infers, that the plane passing by the centre of gravity, and determined according to the preceding conditions, always remains parallel to itself in the movement of the system; a singular advantage, and which renders it of the greatest utility. It is another remarkable circumstance, that every plane parallel to the above, and passing by any one of the bodies of the system, will enjoy analogous properties. After having obtained these valuable results, the author examines the laws of movement which could take place in every possible mathematical relation between the velocity and force. He shows that there exist in this general case, principles analogous to those of the conservation of the living forces, of the areas, of the movement of the centre of gravity, and of the least action in nature. He draws from these results the conditions which essentially distinguish the state of motion from that of equilibrium.—These very remarkable connections are entirely new. The laws of the motions of transposition and rotation of solid bodies are afterwards developed with the greatest extent. The author here demonstrates the properties of the principal axes, and their use in the determination of the *momenta inertiæ:* he searches for the place of the points which remain immoveable during the instantaneous movement of the body; and he is led in a very simple manner to observe, that these points are situated upon a straight line, whence he infers, that every movement of rotation, of whatever kind it may be, is nothing else than a movement of rotation around a straight line fixed during an instant, and variable from one instant to another, a property which has procured it the name of *instantaneous axis of rotation.* The author applies these principles to the case where the movement of the body is owing to a primitive impulsion which does not pass by its centre of gravity: he shows how we may determine the distance of the centre of gravity from this impulsion, when the circumstances of the movement of the body are known, and he gives an example of it drawn from the movement of the earth.

He afterwards considers the oscillations of a body which turns very nearly round one of its principal axes. He demonstrates that this movement is stable around the two principal axes, the *momenta inertiæ* of which are the greatest and the smallest, and that it is not around the third principal axis; so that this last motion may be sensibly affected by the slightest cause. He afterwards integrates the equations which determine the movement of rotation in the hypothesis of the very small oscillations. Finally, he examines the movement of a body subjected to turn around a fixed axis; and supposing this body animated by gravity alone, he determines the length of the simple pendulum which would make its oscillations in the same time. The author afterwards takes up the motion of fluids: he establishes the conditions necessary, in order that this movement may take place, and that the continuity of the fluid at the same time may be always satisfied: he discusses certain cases in which these equations are integrable, such as the case where the density being any given function of the pressure, the sum of the velocities parallel to the three rectangular axes, multiplied each by the element of their direction, forms an exact variation; a condition which will be fulfilled at every instant if it be in one alone. This case takes place when the motions of the fluid are very small; and the author draws from it the equations which involve the theory of the very small undulations of homogeneous fluids. Considering afterwards a homogeneous fluid mass, endowed with a motion of rotation uniform around one of the rectangular axes, he shows that this hypothesis verifies the equations of the movement and of the continuity of fluids; whence he concludes that a similar movement is possible. This case is one of those in which the sum of the velocities multiplied respectively by the elements of their direction is not an exact variation; whence it follows, that motion may take place without this condition being fulfilled.

The author afterwards determines the oscillations of a fluid homogeneous mass, covering a spheroid endowed with an uniform movement of rotation around one of the rectangular axes, supposing this fluid mass to be deranged from the state of equilibrium, by the action of very minute forces: applying these considerations to the sea, and regarding its depth as very small, relatively to the terrestrial radius, he thence deduces the conditions of its motion; and comparing them with those of its equilibrium, he shows that each point of the spheroid covered by the sea is more pressed in the state of motion than in that of equilibrium, from the weight of the small column of water comprehended between the surface of the sea and the surface of level; this excess of pressure becoming negative in the points where the surface is lowered below the level. It results also from the same analysis,

lysis, that supposing the initial velocities and their first differences, divided by the element of the time, had been the same with respect to the molecules situated upon the same terrestrial radius, these molecules will remain upon the same radius during the oscillations of the fluid. The author treats the motions of the atmosphere in the same manner, looking only to the regular causes which agitate it. He first considers it in the state of equilibrium; and comparing the conditions resulting from this supposition with those which the equilibrium of the seas necessitates, from this he infers, that, in the state of equilibrium, the stratum of air contiguous to the sea is every where of equal density; and that the atmospheric strata of equal density are every where equally raised above the level of the sea, with very small exceptions, which, in the exact calculation of the height of mountains by barometrical observations, ought not to be neglected.

The author afterwards examines if it is possible that the molecules of air situated originally upon the same terrestrial radius, still remain upon this radius during the motion which takes place in the oscillations of the sea. He shows that this supposition satisfies the conditions of the motion, and of the continuity of the atmospheric fluid: in this case the oscillations of the various strata of level are the same. These variations of the atmosphere produce analogous oscillations in barometrical altitudes. The author determines them, and shows that they are similar to all elevations above the level of the sea, and proportional to the altitudes of the mercury in the barometer, in the state of equilibrium, at these elevations.

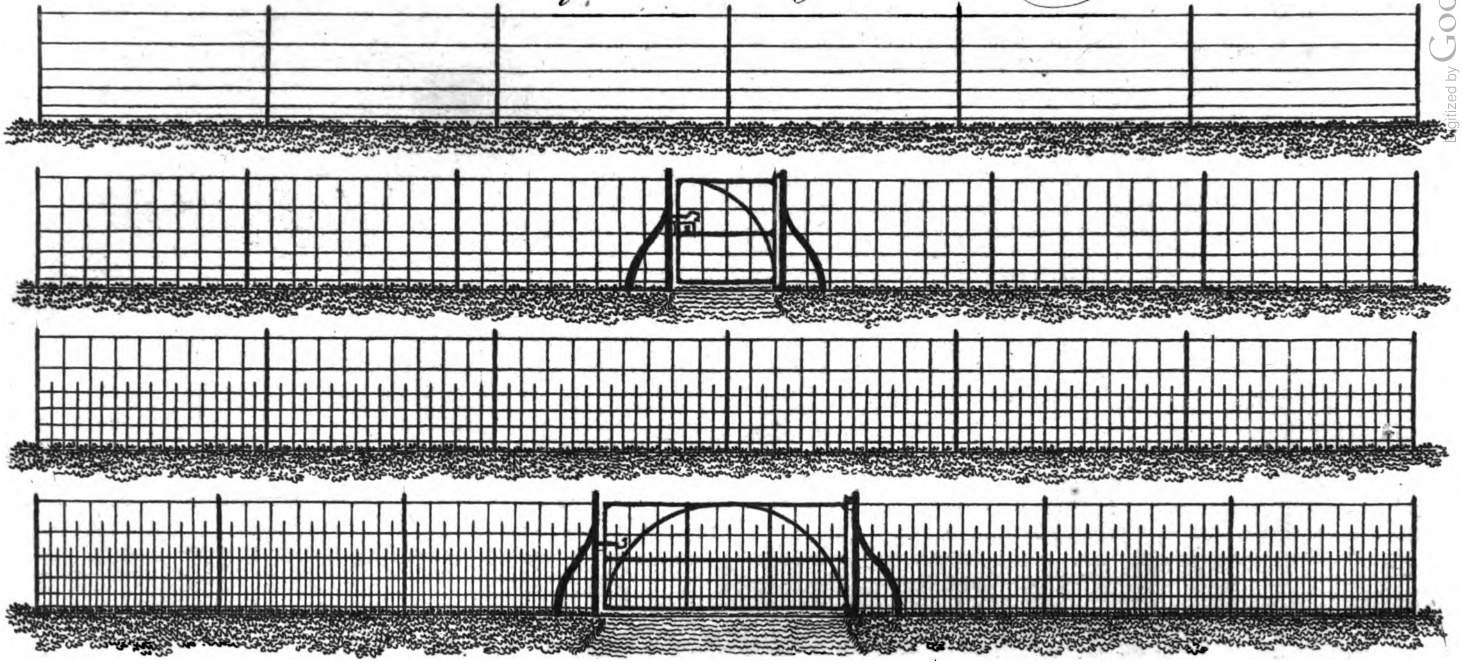
[To be continued.]

XLVII. *Description of a new Fence made of tort elastic Wire, which becomes invisible at a comparatively short Distance, calculated for Pleasure-Grounds.* *By* HENRY HOWELL, *Esq.*

To MR. TILLOCH,—SIR,

SHOULD you deem the following description, and the accompanying plate of a fence for pleasure-grounds, upon a new

Invisible Fence, for enclosing Pleasure Grounds.



Invented & Manufactured by Mr. James Pilton, King's Road, Chelsea, Midd^x.

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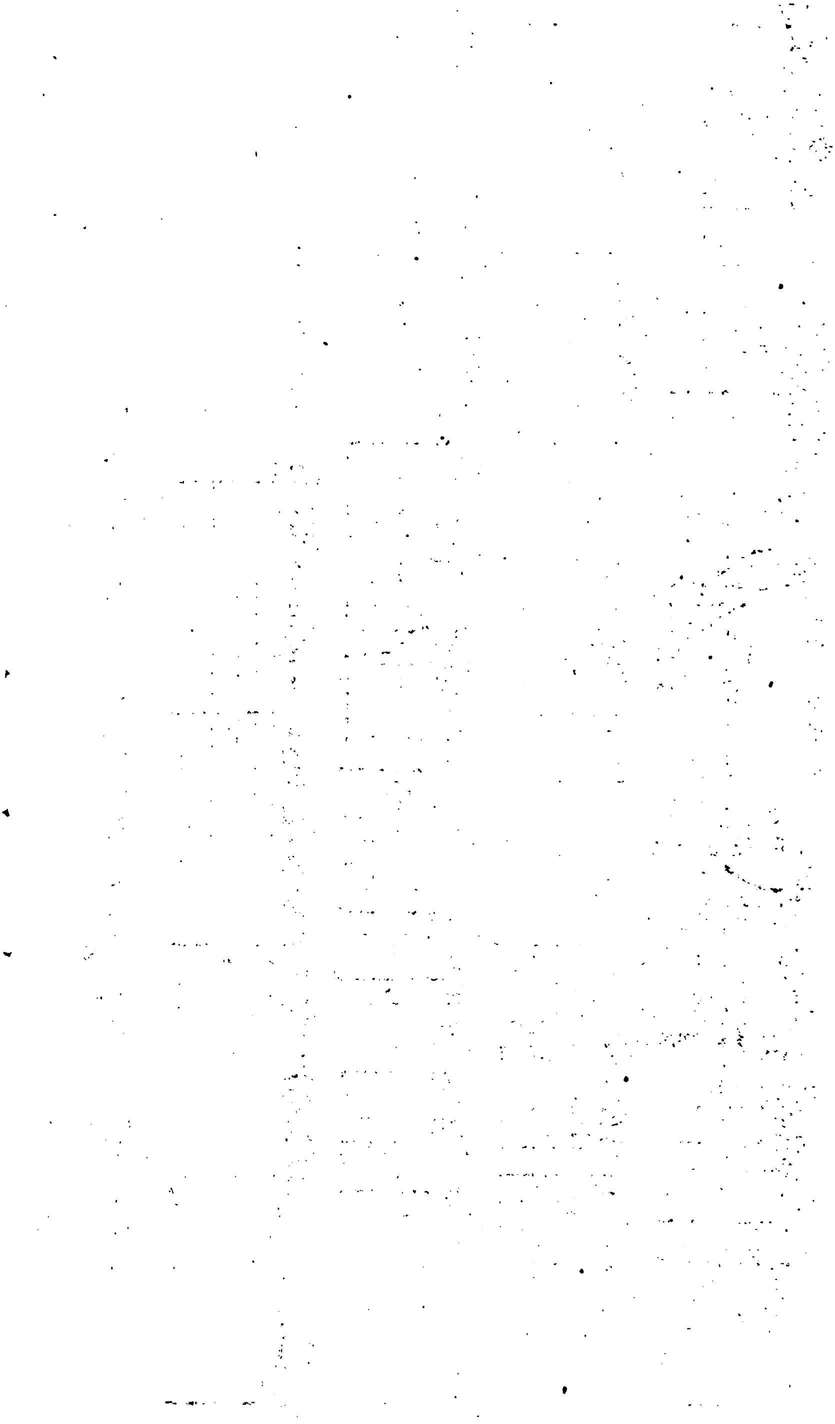

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new principle, at all deserving a place in the Philosophical Magazine, you will oblige me by giving it that distinction.

The basis of the invisible fence is elastic iron wire, manufactured and applied on principles discovered by Mr. James Pilton, King's Road, Chelsea, Middlesex.

This infrangible material for the main wires is drawn out to the thickness of a common quill, of which continuous strings are inserted horizontally through upright iron stanchions: the interval between the strings is about nine inches; between the stanchions, about seven feet. The horizontal wires, in a state of tension, are fastened to two main stanchions at the extremity of the fence, passing at freedom through holes drilled in the intermediate stanchions. The tension of every horizontal wire is preserved by the superior stability of the extreme stanchions; on the construction of which, and the mechanism of the base work, the resistance of the whole, as a barrier against heavy cattle, depends. When the extent of the fence is great, the main stanchions are relieved at expedient distances by other principal stanchions: an improved mode of joining horizontal wires qualifies every part of the length to bear the highest degree of tension.

The invisible fence, in this simple form, of the height of three feet six inches, has, in the Royal Pleasure Grounds at Frogmore, and in various parks of the nobility and gentry, been found adequate to exclude the largest and strongest kinds of grazing stock. Increased in height two feet, the fence becomes applicable to deer parks. Deer have never been known to injure it, or attempt to leap it; from its transparent appearance they probably regard it as a snare.

When it is intended further to keep lambs out of plantations, perpendicular wires, comparatively slight, are interwoven upon the lower horizontal wires; and to protect flowers and exotics from hares and rabbits, it is only necessary to narrow the interstices by minute additions to the upright wires.

On substances so small, presenting a round surface, neither rain nor snow can lodge; independent of which, by a coating of paint they are preserved from the effects of the weather.

The strength attained, by the principles on which the materials are manufactured and the erection of the fence is conducted, cannot be justly conceived but by a person who has witnessed the effect of a considerable force impressed, or weight lodged on a single wire of a fence erected. The tempered elasticity of the tort string allows it to bend, and on the removal of the pressing force, the wire vigorously recoils, vibrating till it reassumes a perfectly straight line; which shows that a violent shock cannot warp it.

With regard to the effect of these transparent boundaries in opening a view, a pleasure-ground intersected or surrounded with them must be surveyed before an estimate can be formed of the small distance at which they vanish from the eye and leave the prospect free;—this distance may be fixed by experience at seventy yards.

To advert a moment to the utility of the new principle, (by which the invisible fence can be rendered strong and durable in any degree demanded,)—from the theory of Mr. Repton, previous to their discovery, it may be collected, that a secure substitute for the heavy and unsightly fences, often found indispensable near the basement windows of a mansion, was a desideratum; and his practice, since the satisfactory trials made in many counties of the new transparent fence, sanctions its adoption. In his large and elegant publication on Landscape Gardening, that able improver of rural scenery states many objections to the Ha Ha; and regrets the necessity for interposing substantial boundaries to a grazed circle near the house, which counteracts a designer in pursuing the incontestably judicious maxim, that the fences in a park cannot be too few. Under a skilful director, the new principle, in the multifold applications of which it is capable, is a powerful instrument in creating artificial beauties round a country residence, or in opening a prospect to adorned nature, where a pleasing fore-ground and enchanting distance have been hitherto shut out. (See the Plate.)

The inventor of these transparent fences has been engaged by Mr. Repton to erect them on several estates distinguished for extent and beauty.

I have the honour to be, sir,
your most obedient humble servant,
Henry Howell.

16, Lower George Street,
Sloane Square.

XLVIII. *On the Affinity existing between Oxides of Carbon and Iron.* By David Mushet, *Esq.*[*]

[Continued from p. 241.]

Experiment X.

Charcoal from Norway Pine,

Composed of oxide of carbon	98·179
Ashes - -	1·821
	100 parts.

15 grains of this charcoal were mixed with 200 grains of oxide of iron. The fusion of this compound afforded a metallic button that weighed 40 grains, equal to 20 per cent.

75 grains of Norway pine, requisite to 15 grains of its charcoal and 200 grains of oxide of iron, produced a metallic button weighing - - - 62 grains.

Revived with charcoal as above -	40
Increase (equal to 11 per cent.)	22

Experiment XI.

Charcoal prepared from Lignum Vitæ,

Composed of oxide of carbon	98·138
Ashes - -	1·862
	100 parts.

15 grains of this charcoal were mixed with 200 of oxide of

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[*] The Reader is requested to correct the following errors in Mr. Mushet's last communication in the present volume:—Page 160, line 2, for 41½ read 14½ grains of iron; page 160, line 11, read *one* part of Lynn sand; page 161, line 9, for *acid* of charcoal read *aid* of, &c.—Also in page 121, for *boiled* read *coiled* up and put into the retort.

of iron. The metallic result from the fusion of this mixture was 38 grains.

55 grains of raspings (equal to 15 grains of the charcoal and 200 of the oxide) gave

A metallic button weighing	58·73 grains.
Revived with charcoal -	38·
Increase (equal to 10·371 per cent.)	20¾

Experiment XII.

Charcoal prepared from Chestnut,

Composed of oxide of carbon	98·20
Ashes - -	1·80
	100 parts.

15 grains of this charcoal and 200 of oxide of iron produced by fusion a metallic button that weighed 40 grains, (equal to 20 per cent.) from the oxide of iron.

83 grains of chestnut wood were found, by a calculation of the loss it sustained in charring, to be equal to 15 grains of charcoal: that quantity, in the state of raspings, was mixed with 200 grains of oxide of iron, and produced by fusion a metallic button (equal to 29 per cent. from the oxide)

Weighing - - - -	58 grains.
Revived as above with charcoal	40
Increase (equal to 9 per cent.)	18

Experiment XIII.

Charcoal prepared from Laburnum,

Composed of oxide of carbon	95·20
Ashes - -	4·80
	100 parts.

15 grains of this charcoal and 200 of oxide of iron yielded a metallic button of iron weighing 41 grains (equal to 20½ per cent.).

73 grains of laburnum (being found equal to 15 grains of coal) and 200 grains of oxide of iron, mixed intimately together, produced a metallic button (equal to 25½ per cent.)

Weighing

Weighing - - - -	52 grains.
Revived with charcoal -	41
Increase (equal to 5½ per cent.)	11

Experiment XIV.

Charcoal prepared from Scotch Oak,

Composed of oxide of carbon	98·135
Ashes - -	1·865
	100 parts.

15 grains of this charcoal and 200 of oxide of iron were intimately mixed, and fused together. A button of iron was obtained that weighed (equal to 27 per cent.) 54 grains.

65 grains of oak being requisite to form the above, 15 grains of this charcoal were mixed with 200 grains of oxide of iron. The fusion of this mixture was productive of a button of iron that was found to weigh (equal to 31½ per cent.)

cent.) - - - - -	63 grains.
Revived with 15 grains of charcoal	54
Increase (equal to 4½ per cent.)	9

Experiment XV.

Charcoal prepared from the White Wood of the same Oak,

Composed of oxide of carbon	97·325
Ashes - -	2·675
	100 parts.

15 grains of this charcoal and 200 grains of oxide of iron yielded a metallic button that weighed 49 grains (equal to 24½ per cent.).

96 grains of white wood, found by calculation from experiment to be equal to 15 grains of charcoal, were mixed with 200 grains of oxide of iron, and the compound reduced by fusion. The result was a metallic button that weighed

(equal to 34½ per cent.) - - -	69 grains.
Revived with charcoal -	49
Increase (equal to 10 per cent.)	20

Experiment XVI.

Charcoal prepared from Ash,

Composed of oxide of carbon	95·727
Ashes - -	4·273
	100 parts.

15 grains of this charcoal and 200 grains of oxide of iron were fused together, from which was obtained a metallic button that weighed 54 grains, or 27 per cent., from the oxide of iron.

80 grains of ash wood was found equivalent to the formation of 15 grains of this charcoal. To these were added 200 grains of oxide of iron. The fusion of the compound produced a metallic button of iron weighing (equal to 32·75 per cent. from the oxide) - - 65·5 grains.

Revived with the charcoal	54
Increase (equal to 5¾ per cent.)	11·5

Experiment XVII.

Charcoal prepared from Bark of the same Ash,

Composed of oxide of carbon	93·55
Ashes - -	6·45
	100 parts.

15 grains of this charcoal were mixed with 200 grains of oxide of iron and fused together, and there resulted a metallic button of iron that weighed 41 grains (equal to 20½ per cent.).

78 grains of this bark were found requisite to form the above portion of 15 grains of charcoal. That quantity was therefore thoroughly mixed with 200 grains of oxide of iron. A metallic result was obtained by the fusion of the compound, and the resulting button weighed (equal to 33½ per cent.) - - - - - 67 grains.

Revived with charcoal as above	41
Increase (equal to 13 per cent.)	26

Experiment XVIII.

Charcoal prepared from Birch,

Composed of oxide of carbon	89·681
Ashes -	10·309
	100 parts.

15 grains of this charcoal and 200 grains of oxide of iron yielded by fusion a metallic button of cast iron that weighed 62 grains (equal to 31 per cent. from oxide).

90 grains of birch-wood, being found equivalent to the above 15 grains of charcoal, were mixed, in the state of raspings, with 200 grains of oxide of iron. The result on fusion was a button of iron

Weighing (equal to 33 per cent.)	66 grains.
Revived by means of the charcoal	62
Increase (equal to 2 per cent.)	4

Experiment XIX.

Charcoal prepared from Sycamore,

Composed of oxide of carbon	94·593
Ashes - -	5·407
	100 parts.

15 grains of this charcoal being mixed with 200 grains of oxide of iron and fused, a metallic result was obtained that weighed 50 grains (equal to 25 per cent. from oxide of iron).

79 grains of sycamore raspings (equal to 15 grains of charcoal) and 200 grains of oxide of iron yielded by fusion a metallic button that weighed (equal to 31½ per cent.) 63 grains.

Revived by means of the charcoal	50
Increase (equal to 6½ per cent.)	13

Experiment XX.

Charcoal prepared from Lime-tree,

Composed of oxide of carbon	96·321
Ashes -	3·679
	100 parts.

15 grains of the above charcoal and 200 of oxide of iron gave a metallic button weighing 51 grains (equal to 26½ per cent. from oxide of iron).

83¼ grains of raspings, as requisite to form the above quantity of charcoal, and 200 grains of oxide of iron, being mixed and perfectly reduced, afforded a button of iron that weighed (equal to 34½ per cent.) - 69 grains.

Revived by means of charcoal	51
Increase (equal to 9 per cent.)	18

Experiment XXI.

Charcoal prepared from Bragnut of the specific gravity of 1·1009,

Composed of oxide of carbon	96·250
Ashes -	3·750
	100 parts.

15 grains of this charcoal were mixed with 200 grains of oxide of iron, and the result by fusion was a metallic button weighing 36 grains (equal to 18 per cent.).

45 grains of bragnut were found equivalent to form 15 grains of charcoal. These in a state of raspings, and 200 grains of oxide of iron, were mixed together and fused; the result was a metallic button of iron weighing (equal to 21 per cent.) - - - - - 42 grains.

Revived by means of charcoal	36
Increase (equal to 3 per cent.)	6

From the result of these experiments, it will appear at one glance, that the extent and purity of the carbonaceous matter in charcoals do not at all depend upon the absolute quantity of combustible matter they contain respectively.

The largest portion of revived iron is obtained with charcoal of birch, the quantity of combustible matter in which (by Experiment XVIII.) is 89·681. Iron revived 62 grains.

Combustible matter in oak	98·135	- 54
In ash	95·725	- 54
In walnut	96·048	- 36

This contrast is quite sufficient to show that the different carbonating powers of charcoal of wood depend upon a principle different from any that has been developed in the foregoing experiments.

From the same experiments, however, we are warranted to conclude, that the carbonating powers of the matter of carbon contained in different woods in their natural state, are greater than when the same is reduced to charcoal by distillation or any other mode of operation. This curious fact, the reverse from what might have been expected, may be accounted for in three different ways.

1st, From the decomposition of the oleaginous or resinous juices of the wood by the oxide of iron; part of the carbonaceous matter of which, being set free, may either unite itself to the iron, or unite with the oxygen of the oxide, and by this means leave greater scope to the carbonating powers of the concrete carbon.

2dly, From a large surface being exposed by wood in the state of fine sawdust to the same bulk and weight of oxide.

3dly, And what seems to be the most permanent cause, this fact may arise from a certain degree of oxidation being necessary in the carbon, which facilitates its union with the oxygen of the oxide; and as the degree of oxidation in raw wood is greater than in charcoal, so in proportion to this degree of oxidation we find the affinity more speedily and more extensively exerted.

The following Table will prove a convenient summary and contrast of the foregoing experiments.

Table of 21 Experiments made with different Kinds of Woods.

No. of Exper.	Names of the Woods.	100 Parts of Charcoal of each Wood composed of		Quantities of Iron revived with 15 Grs. of Charcoal.		Weights of Wood in grs. requisite to form 15 grs. of Charcoal.	Quantities of Iron revived with Wood in the state of Raspings		Increase of Iron with Raw, beyond that of Coaled Wood.	
		Oxide of Carbon.	Ashes.	Weight in Grains.	Per Cent.		Weight in Grains.	Per Cent.	Weight in Grains.	Per Cent.
1	Walnut	96·048	3·952	36	18	76	49	24½	13	6½
2	Elm	96·700	3·300	40	20	75	59	29½	19	9½
3	Holly	94·152	5·845	44	22	71¼	45	22½	1	½
4	Scotch Pine	97·100	2·900	40	20	88	68	34	28	14
5	Beech	95·200	4·800	42	21	54½	54½	27¼	12½	6¼
6	American Maple ..	96·140	3·860	50	25	76	61	30½	11	5½
7	Spanish Mahogany	96·154	3·846	40	20	55½	43	21½	3	1½
8	Sallow	93·865	6·135	43	21½	79	60	30	17	8½
9	American Black Beech	95·165	4·831	36	18	69	48	24	12	6
10	Norway Pine	98·179	1·821	40	20	75	62	31	22	11
11	Lignum Vitæ	98·138	1·862	38	19	55	58¾	29⅓	20¼	10·3
12	Chestnut	98·200	1·800	40	20	83	58	29	18	9
13	Laburnum	95·200	4·800	41	20½	73	52	26	11	5½
14	Scotch Oak	98·135	1·865	54	27	65	63	31½	9	4½
15	White Wood of Do.	97·325	2·675	49	24½	96	69	34½	20	10
16	Ash	95·727	4·273	54	27	65	63	31½	9	4
17	Bark of Ash	93·550	6·450	41	20½	78	67	33½	26	13
18	Birch	89·631	10·309	62	31	90	66	33	4	2
19	Sycamore	94·593	5·407	50	25	79	63	31½	13	6½
20	Lime-tree	96·321	3·679	51	25½	83¼	69	34½	18	9
21	Bragnut	96·250	3·750	36	18	45	22	21	6	3

[To be continued.]

XLIX. *On*

XLIX. *On the native Gold Dust found in the Hills in the Environs of the Commune of St. George, in the Department of Le Loire.* By Mr. GIULIO, *Prefect of the Department of the Sesia**.

IT has long been known that a great number of rivers and rivulets carry with them particles of native gold, of larger or smaller size; that independently of the places where this metal is found in its matrix, it is disseminated in grains in their sands, as those of the Rhone, the Arriège, and the Cèze in France, and with us in those of the rivers Loire, Balthée, Cervo, Elbo, Mallon, and Orba, and of the rivulets Oropa, Orémo, Evançon, Vison, &c. It is equally known that there are persons who make it their whole business to search for this gold, who are called, in the language of the country, *arpailleurs*, *orpailleurs*, or *pailloteurs*.

Mineralogists are not agreed respecting the origin of these gold grains: the older mineralogists, and Brochant among the moderns, maintain that this gold is washed by the currents from its native mines, commonly situated in primitive mountains. "Native gold," says Brochant †, "is found chiefly in primitive mountains, where it is met with in veins, and sometimes disseminated in the rock: it occurs also in alluvial strata, where it is frequently wrought with advantage. The sand of several rivers is mixed with grains of gold, which are separated from it by washing. It is unquestionably evident, that the gold here is met with accidentally; and that it is deposited by the water that has washed it away from its original situation, which was probably the same as is indicated above."—Others think that these metallic particles were originally disseminated in auriferous strata, in the very places where they are exposed to view, by great floods, or overflowings of the rivers, or that they have been washed into the latter by torrents in storms or heavy rains.

I do not mean to enter into the question at large. This I leave to the learned, whose chief study is the improvement of the science of mineralogy. My inductions go no further than the small number of researches I have made: yet I think I may venture to say, from the observations I am about to present to the reader respecting the locality and situation of the native gold dust in the commune of St. George, that such dust is not always washed down from mines in the mountains by rivers. And if such were the primitive origin of their dissemination amid the strata, it certainly could have happened only at some very remote period of the grand disruptions that have taken place on the surface and exterior of the strata of our globe. But these revolutions, of which we have no records, are buried in the night of time. For we shall see that strata which furnish gold dust are found at a considerable depth in some hills, equally remote from mountains capable of furnishing it, and from rivers that could force it from its native situation. It could, therefore, have mingled in them only at a remote period, when the strata of the hills assumed the arrangement they have at present, namely, at the time of their formation.

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* From *Journal des Mines*, vol. xx.

† Elementary Treatise on Mineralogy, according to the Principles of Prof. Werner, vol. ii.

This has been the opinion of several naturalists of our country, and I should be guilty of injustice to them, if, in collecting fresh proofs tending to support their hypothesis, I omitted to mention their valuable works. Accordingly I shall quote Mr. de Robillant, who, speaking of the gold dust found in the sands of the Orco, says very positively: "This river carries along gold, which the people of the country observe only below the bridge down to the Po; which confirms the opinion held by the people best acquainted with the natural history of the country, that it is from the gullies and hills that this gold dust is washed down into the river by the rapidity of the water during storms *. This valuable metal does not come from the high mountains, since none is found above the bridge; but it originates from the washing of the red earth, of which most of these hills and plains are composed, and which in stormy weather is carried down into the principal river †."

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* See a geographical Essay on the Continental Territories of the King of Sardinia, by de Robillant, in the Memoirs of the Royal Academy of Sciences of Turin for 1784-5, part ii. p 234.

† Ib. p. 268.

Mr. Balbo agrees with M. de Robillant respecting this species of native gold, in his learned Memoir on the auriferous sand of the Orco. "Every one," says he, "knows that gold dust is collected in the Orco.—But I do not believe it is equally known, that gold is found, not in the bed of the river alone, but to the distance of several miles, every where mingled more or less with the sand.—It is very positively asserted that it occurs in all the little rivulets between Valperga and Rivara.—I endeavoured to discover whether all the waters rise sufficiently near to each other to lead us to suppose that they equally derive their gold from the same mine; as it is in this way that the vulgar, and even most of the learned, generally account for the gold found in rivers. But I was completely convinced that the waters of which I speak arise from different heights at some distance from one another; so that, as we cannot suppose all these places to contain mines, from which the gold may be derived, we must necessarily admit that the particles of gold are not separated daily by the action of the water, and carried along by its streams, but that the water finds them in the soil itself over which it flows.—And it is further confirmed by the observation, that the auriferous strata disappear as we proceed up the Orco; that we find them at furthest only as high as the bridge; that above this all traces of them are lost, though this is very far from the springs; while as we descend into the plain these strata are every day exposed by the action of the water, and particularly in floods*."

In a second part I shall speak of the theory proposed by M. Napion, in his Memoir on the mountains of Canavais †, who, having observed that all the pyrites of those mountains are auriferous, attributes the particles of gold to their decomposition or attrition. This is the opinion of our worthy colleague, Dr. Bonvoisin.

The observations I am now about to communicate appear to me still more decisive than the proofs alleged by these authors; and if the earths of which I shall speak do not furnish so large a quantity of gold dust, they afford indisputable proofs that the gold certainly does not proceed from any mine traversed by water, at least in the present day.

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* Mem. of the Roy. Ac. of Turin for 1784-5, on the auriferous Sand of Orco, part ii. p. 404—407.

† Ib. for 1785-6, p. 345-6.

In the north of the commune of St. George, in the circle of Chivas, in the department of the Loire, we find fertile rising grounds, and hills almost wholly covered with vineyards, which continue till we come to the highest of them, the hill of Macugnano, part of which is cultivated, part covered with wild chestnut trees; a distance of about three miles.—In our progress from the outer and upper surface of these hills to the bottom of the valleys, which intersect them in different directions, we find in general three very distinct strata.—The upper stratum is for the most part argillaceous, as it furnishes an excellent earth for making bricks and tiles. The thickness of this stratum varies in different places from three or four feet to twenty-five or thirty. The second stratum, which stretches likewise horizontally beneath the stratum of clay, is a few feet thick. It is composed of a considerable portion of sand, of gravel, and of pebbles of different natures, argillaceous, calcareous, and quartzose. Of these I shall speak more particularly in the second part, as well as of the fragments produced by their being broken or decomposed. The third or lower stratum, which forms the bed of the valleys, and of the rivulets that run through them in rainy weather, is composed in great measure of the fragments of the argillaceous and calcareous stones of the second stratum.—The rains have gradually produced little gullies in different directions; which by the falling of fresh rain, and the quantity and rapidity of the water, have in the course of time been extended and converted into valleys, more or less broad and deep, in different places. Part of the water of several gullies accumulates particularly in one valley, where during storms and long rains it forms a torrent, called in the country the Merdanzone. Now the gold dust is found chiefly among the sands of this torrent, and of the small lateral rivulets that flow into the Merdanzone or other similar valleys.

Does this gold proceed equally from the different strata I have mentioned above, or from one of them only? I first examined the brick earth (that of the upper stratum) in different places and at various depths: I also examined considerable depositions of this earth accumulated in the shallow valleys: but I never discovered the smallest particle of gold in it. The searchers for gold know this so well by long experience and a great number of fruitless trials, that they never pay any regard to this stratum. It is the stratum beneath the argillaceous composed of gravel, sand, micaceous and calcareous stones, in which the particles of gold are found.

Of this I have convinced myself by several trials: and though in general, if equal quantities of earth be taken from this stratum, and from the bed of the torrent or rivulets flowing into it, the latter will yield most gold, it seldom or ever happens that no gold is found in the former upon trial. The particles of gold obtained from the auriferous stratum itself, which have not yet been rolled along with the sand by the rains, have a duller and deeper yellow colour than those collected in the bed of the torrent or of the rivulets, which are of a more shining yellow, no doubt in consequence of the attrition. They are generally found amid a sand that is more or less fine and blackish, and apparently of a siliceous and ferruginous nature. The earth of the same nature, which reaches to some distance, equally contains gold. Thus a brook that runs on the east of the commune of Aglie, between the mansion and the park, and receives the rain water that washes down an earth composed of different strata of the same nature as those of the auriferous hills of St. George, equally rolls along particles of gold disseminated beneath the argillaceous stratum, which in some places is of very considerable thickness.

Between fifteen and twenty years ago several persons in the commune of St. George made it their principal employment to search for gold in the sand of the torrents and rivulets that I have mentioned. This they did particularly after or during heavy rains, and after storms.

The quantity of gold they collected in a day was very variable. Sometimes each of them would gain eight or ten shillings a day, at other times scarce a fourth or fifth of this sum. The size of the particles too varied much, from an almost invisible atom to the weight of nine or ten grains or more. They were afterward sold to merchants, who sent them to the mint.

I do not speak here of gold dust disseminated in arable land. Earth of this kind in the territory of Salussole, as I am informed by my colleague Giobert, contains particles of gold. The earth of gardens is known to contain them. It has been proved in our days by the experiments of Sage, Berthollet, Rouelle, Darcet, and Deyeux, that there are particles of gold in vegetables. Berthollet has extracted about 2·14 gram. (33 grs.) from 48900 gram. or a hundred weight of ashes.

Gold has not yet been found in the arable land in the environs of St. George, but only in the stratum beneath the clay, the surface of which is cultivated. The auriferous stratum, as I have observed, is more than thirty feet deep below the argillaceous stratum in some places.

We have nothing to do here with particles of gold mixed with the surface mould by the decomposition of plants, or which plants have derived from the earth. I have no doubt that the particles of gold found in the environs of St. George have the same origin as those met with from Pont to the entrance of the Orco and of the Mallon into the Po, from Valperga and Rivara, to Aglie and St. George's; as well as of those which Dr. Bonvoisin observed in the environs of Challant in the valley of Aoste. The famous piece of native gold preserved in the arsenal was found there. In that space, pieces of gold of the weight of a louis have sometimes been found; and other pieces are mentioned of the value of more than 100 livres (4*l*. 3*s*. 4*d*.). Probably the gold found in the earth in the valley of Brozzo, and in other places, has the same origin. My conjectures on this subject shall be proposed in the second part of this memoir, where the nature of the earths and stones of the auriferous strata, as well as the nature of the land in which they are contained, shall be entered into more at large.

L. *Some*

L. *Some Circumstances relative to Merino Sheep, chiefly collected from the Spanish Shepherds, who attended those of the Flock of Paular, lately presented to His Majesty by the Government of Spain; with Particulars respecting that great National Acquisition; and also respecting the Sheep of the Flock of Negrete, imported from Spain by His Majesty in the Year 1791.* By Sir JOSEPH BANKS.

[Concluded from p. 248.]

IN the year 1787 the king, guided by those patriotic motives which are ever active in his majesty's mind, gave orders for the importation of Merino sheep for his own use, and for the improvement of British wool. As it was doubtful at that time whether the king of Spain's license, without which these sheep cannot be embarked at a Spanish port, could be obtained, it was deemed advisable to make the first purchases in the parts of Estremadura adjoining to Portugal, and to ship the sheep for England at Lisbon. The first importation of these valuable animals arrived in March 1788, and a little flock of them was soon after completed; but as these were of various qualities, having been drafted from different Cavañas, his majesty was pleased to order an application to be made to the king of Spain by lord Auckland, then his majesty's minister at that court, for permission to import some sheep drafted from one of the prima piles. This was obtained; and a little flock, consisting of 36 ewes, 4 rams, and 1 manso, arrived safe and well at Dover, in 1791. These sheep had made a part of the Cavaña called Negrete, one of the three piles restricted from exportation, and which is likewise remarkable for producing the largest-carcased sheep that are to be found among the Merino flocks, as has been before stated.

On the receipt of this treasure, (for such it has since proved itself to be,) the king, with his usual prudence and foresight, ordered the whole of the sheep that had been procured by the way of Portugal to be disposed of, (which was immediately done,) and directed the Negrete breed to be increased as much as possible, and maintained in its utmost purity.

From that time to the present the opinion of the public, sometimes perhaps too unwary, and at others too cautious, in appreciating the value and adopting the use of novel kinds of sheep, has gradually inclined to give that preference to the Merinos which is so justly their due. At first it was impossible to find a purchaser willing to give even a moderate price either for the sheep or for their wool; the shape of the sheep did not please the graziers, and the wool-staplers were utterly unable to judge of the merit of the wool, it being an article so many times finer and more valuable than any thing of the kind that had ever before passed through their hands. The butchers, however, were less timorous; they readily offered for the sheep, when fat, a fair mutton price; and there are two instances in which, when the fat stock agreed for was exhausted, the butcher who had bought them anxiously inquired for more, because he said the mutton was so very much approved of by his best customers.

It was not, however, till the year 1804, thirteen years after their first introduction, that it was deemed practicable to sell them by auction, the only certain means of placing animals in the hands of those persons who set the highest value upon them, and are consequently the most likely to take proper care of them. The attempt, however, succeeded; and the prices given demonstrated that some at least of his majesty's subjects had at that time learned to put a due value on the benefit his royal patriotism offered to them. One of the rams sold at the first sale for 42 guineas, and two of the ewes for 11 guineas each; the average price at which the rams sold was 19*l.* 4*s.*, and that of the ewes 8*l.* 15*s.* 6*d.* each.

This most useful mode of distribution has since that time been annually continued, and the sales have taken place in the beginning of August. The last sale was held on the 17th of August, 1808; when the highest price given for a ram was 74*l.* 11*s.*, for an ewe 38*l.* 17*s.* The average prices of rams was 33*l.* 10*s.* 1*d.*, of ewes 23*l.* 12*s.* 5*d.*;—a most decisive proof not only that the flock had risen very materially in public estimation, but also that the sheep have not in any way degenerated from their original excellence.

The

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The wool was at first found to be quite as difficult of sale as the sheep themselves; manufacturers were therefore employed to make a considerable quantity of it into cloth, which, when finished, was allowed by both woollen-drapers and tailors to be quite as good as cloth made of wool imported from Spain. But even this proof would not satisfy the scruples of the wool buyers, or induce them to offer a price at all adequate to the real value of the article: it was found necessary, therefore, to have the wool scoured, and to sell it in that state as Spanish wool, which, though grown in England, it really was. Thus managed, the sales were easily effected for some years, at a price equal to that demanded for the prima piles of imported Spanish wool at the times when the bargains were made.

Time and patience have at last superseded all difficulties, and his majesty's wool has now for some years been sold as clipped from the sheep's backs, the sheep having been washed, and the whole management of them carried on exactly in the English manner, at a price not lower than 4*s*. 6*d*. a pound, which, allowing for the loss of weight in the scouring, costs the buyer at least 5*s*. 6*d*. a pound, a tolerable price for Spanish wool when plenty of it could be produced, though not possibly so high an one as ought to have been given or as will be obtained for the Anglo-Negrete pile, when the value of the article is fully understood.

The race of another capital Cavaña has now been added to the riches of this country, the Paular, and the draught from it is larger than on any other occasion has been suffered to leave Spain; the animals have been selected with skill and attention, the pile they belong to stands at the very top of our English list, and the sheep have been most fortunately placed at the disposal of our most gracious king, whose shepherds have demonstrated to the public, in an experience of 17 years of their management of these interesting animals, that they can not only continue the breed in its original purity, but can also preclude all danger of degeneration in the article of wool. What more can be wished for on this head?

That spirit of patriotism, which induced our sovereign to declare himself the protector of the purity of the Negrete race, will also, it is most earnestly to be hoped, induce his majesty to extend the same protection to the newly arrived Paulars; by this measure, and by this alone, the public will be effectually guarded against all danger of the admission of impure blood, which the avarice of ill-judging individuals, seeking after a premature improvement of the carcase, has too often, it is feared, introduced into our English flocks. Thus protected, the twofold treasure obtained for the advantage of his subjects by his majesty's wisdom and foresight, will become a perennial fountain of true Merino blood, to which those agriculturists who are wise enough to adopt the breed may from time to time resort, to correct their errors if they fall into bad practices, to carry on their crosses, if any such are found to be advantageous, to the highest degree of perfection, and to restore the originality of their stock, if, in consequence of any unsuccessful experiment, it should have suffered deterioration.

LI. *Remarks on M.* BURCKHARDT'S *Contrivance for shortening Reflecting Telescopes; with a new Method of making Refracting Telescopes with a Tube only one-third of the focal Length of the Object-glass. By* DAVID BREWSTER, *LL.D. F.R.S., and F.A.S., Edin.*

DEAR SIR,

IN the *Connaissance des Tems* for 1809, I observe the description of a new telescope, invented by the celebrated M. Burckhardt, of an intermediate nature between the Gregorian and Newtonian telescopes; and requiring a tube only half as long as other instruments of the same focal length. The large concave speculum AB, Fig. 1, (Plate IX.) is perforated as in the Gregorian telescope, so that the diameter of the aperture *ef* may be half the diameter AB. The parallel rays R*m*, R*n*, which, after reflection, would have formed the image at F, are intercepted by a plane speculum C, perpendicular to the axis of the telescope, and of the same size as the aperture *ef*, and are reflected back to the point *o*, where an image is formed, and viewed with the eye-glass G. By giving a small inclination to the plane speculum, the image may be formed at A, above the speculum, without a perforation in its centre.

The merit of this invention does not exclusively belong to the French astronomer. The same principle was employed in the construction of the reflecting telescope more than six years ago by Mr. Paterson, who had then the management of the Observatory at Edinburgh, and instruments constructed in this manner were advertised for sale in that city. In order to try the performance of a telescope made on this principle, I constructed a small one several years ago, without perforating the concave speculum; but the great loss of light when the plane speculum intercepted the incident rays that would otherwise have fallen on the central and the most perfect part of the concave one, and the distortion of the image when the great speculum was inclined a little and the plane one placed out of the incident rays, induced me to think that the convenience which arises from shortening the tube could by no means compensate for the disadvantages with which it was necessarily attended. This was also the opinion of the late celebrated Dr. Robison, to whom Mr. Paterson showed one of his new telescopes. M. Burckhardt, however, seems to think that the advantages resulting from shortening the tube greatly preponderate; and that the loss of light, which I have before stated, may be remedied by enlarging the aperture of the concave speculum. The Board of Longitude at Paris have seconded the views of M. Burckhardt, and have ordered a telescope to be constructed on the principle already described.

As this subject has been taken up by the French astronomers, it may not be uninteresting to give an account of two other contrivances which have occurred to me for shortening the tubes of telescopes. If instead of the plane speculum C, we substitute a convex one so as to form an image at O, the telescope becomes Cassegrainiam, requiring a tube only about one-half the focal length of the concave speculum. This construction is preferable to that which is described by Burckhardt; because it is much easier to give a correct figure to a convex than to a plane speculum; and it is well known to practical opticians, that the errors of one spherical speculum often correct those of the other.

If any real advantages arise from shortening the tubes of reflecting telescopes, it becomes a matter of importance that refracting telescopes should possess similar properties. By means of the following contrivance the tubes of refractors may be so much shortened as to be only one third of the focal length of the object-glass. A plane speculum CD, whose diameter is two-thirds of that of the object-glass AB, is placed that CA is one-third of the focal length of AB. By giving a small inclination to CD, the rays are reflected to a second plane speculum EG, equal to one-third of the diameter of AB, which again reflects the incident rays to F, where the image is formed and magnified by the eye-piece. In this construction, the only disadvantage is the loss of light occasioned by two reflections; but this may be obviated by increasing the aperture of the object-glass, and is by no means such a serious evil as that which arises in M. Burckhardt's contrivance, from the loss of such a large central portion of the great speculum.

I am, dear sir, your most obedient servant,

D. Brewster.

To Mr. Tilloch.

LII. *A Reply to Earl Stanhope, on his Defence of certain Principles and Facts erroneously stated in his Stereotyped "Principles of the Science of Tuning Instruments with fixed Tones."* By Mr. John Farey.

> "The difference between a man of *real* science, and one who has the ambition to be thought so, is very great."—Earl Stanhope.

To Mr. Tilloch,—Sir,

The truths and principles of the Mathematical Sciences are not in any instance to be yielded to authority, however imposing its aspect; neither should we suffer any other considerations, long to restrain our efforts, in defending their just cause. It has been purely out of regard and tenderness to the unfortunate situation of a musician of the very first rank, whose mental aberrations had been much aggravated by the part he was led to take, and made *appear* to act, in explaining and defending a noble Earl's reveries on the subject of Tuning musical Instruments, that I have been so long kept back from replying to such parts of the two Letters of Earl Stanhope, printed in your Magazine (vol. xxviii. p. 144, and xxx. p. 34,) as relate to the scientific principles of Tuning: and similar feelings towards the very respectable individual alluded to, alone induce me to refrain from again touching on the two " Plain Statements," and the "Narrative," further than to declare, as in justice to Dr. C.'s musical reputation I think I ought, that he never, I believe, perused or saw the Stanhopian " Plain Statement," mentioned by His Lordship in vol. xxx. p. 25, *previously to its publication**, except in the hands of Mr. Ferguson, from whom he *refused to take the proof sheets, or look at them*: but, as Mr. F. himself told me, directed him to take them again to the printer; intending, as he (Dr. C.) has often told me, that His Lordship should be responsible for what he had written and got printed, and not suspecting, under the circumstances, that the name of *J. W. Calcott* would be affixed to it when published. After this, there needs no more for me to say at present, than request those who happen to have the two pamphlets, to *compare them together*, as the worthy and unfortunate Doctor intended, by stitching up and distributing them, as I mentioned in a former communication.

There are six *questions* touched upon in His Lordship's two Letters referred to, on each of which I wish to be indulged in saying a few words:—these are shortly,

1st, Whether a monochord board should be divided into 120 or 100 parts?

2d, Whether the *difference of the lengths* of string, can *accurately measure the interval* between the sounds of two strings, of the same size, weight, and tension?

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* Indeed I saw Dr. C. write a Note to you, Sir, to this effect, in February 1808, with an intent that you should *publish* this fact in your Magazine.

3d, Whether *four* or *five columns* are to be found in pages 7 and 22 of the Stereotype pamphlet?

4th, Whether the intervals called the four tierce wolves are of *the same* or *different magnitudes?*

5th, Whether *equal temperaments* of successive concords of the same kind, produce *equality in the rates of their beating?*

6th, Whether the notation of musical intervals generally, by Σ, f and m, rather than by their most simple ratios, be analogous to substituting a notation by *scores, dozens*, and *odd*, in place of the universally received *decimal notation?*

I.—In addition to the reasons I have given at page 192 of vol. xxvii., for preferring a decimal division of the monochord, I have further to remark, on what has fallen from His Lordship (page 144 of vol. xxviii.) that those "important lengths" which His Lordship's scale is calculated to show *in round numbers*, are perfectly *unimportant*; for, what person using a monochord, other than as a play-thing, wants to use the scale attached to the string at all, in tuning a *perfect concord* of any kind? And does not the use of its scale as a tuning apparatus wholly consist, in either setting or taking off *tempered intervals?* And whether is it easiest, to set a triequal quint for instance, on a decimal scale by my number ·6694829, or on His Lordship's scale of 120, by means of his vulgar fractions $\frac{2,00898,850+}{3,00000000}$ (Stereotype page 23,) or $\frac{71,70,247,592+}{107,10,927200+}$ $\left(\frac{A}{D}\text{ p. } 21\right)$?

II.—I have maintained (and am backed by all mathematical writers) that it is *ratios* only, and not *lengths*, except of such things as in their nature measure ratios, as logarithm scales &c. do, that can define musical intervals. And though His Lordship expressly asserts (p. 145, vol. xxviii.) that "deviations from perfect intervals are concisely, as well as accurately and conveniently expressed, by means of the *difference of the lengths* of wires," I shall take the very example which he alludes to, (Stereotype p. 8,) wherein it is said, that 1·44 the *difference* of two strings, of which the octave length is 120·00, "shows the value" of the tierce wolf; in order to show, that $\frac{144}{12000}$, instead of expressing an interval called the enharmonic *diesis* (21 Σ + 2 m) as it ought to do, represents an interval exceeding 6 octaves by a superfluous third (3905 Σ + 77 f + 338 m)!

III.—Five columns certainly appeared to my eyes, when I was commenting on the Stereotype pages 7 and 22, therefore, unluckily it should seem, I mentioned *five*; but have I anywhere said or insinuated, that His Lordship therefore intended to represent *five wolves*, besides that produced by the quints? And I could not myself have intended to represent five such wolves, when his Lordship is severe upon me for saying there are but two in all. His Lordship's sarcasms, about dividing 12 into 5 aliquot parts, might therefore have been spared.

IV.—My arguments for the exact equality of all His Lordship's four tierce wolves, (at page 200, vol. xxvii.) retain their force, and are not invalidated by what His Lordship has advanced at page 149, vol. xxviii.; where, fortunately, His Lordship has let us into the secret of his blunders in this respect, by the mention of "monochord lengths," showing, that when His Lordship argues for *as complete a distinction* between his tierce wolves, *as to magnitudes*, as between half-guineas, half-crowns, sixpences, and halfpence, he had no better ground than their different lengths on the monochord; forgetting what I had endeavoured to impress on him, under the second head above, as to the fallacy of these lengths as a test of the magnitudes of intervals. Could not His Lordship as easily "distribute" or divide *the same* interval in four different ways in his C G D and A columns, as he can so distribute four *different* intervals? unless he confines his idea of equality, to monochord lengths, as then of course, they would only fit where the *octave* and *thirds* are also of the proper proportionate lengths! Absurdities, to which His Lordship surely could not have turned his attention.

V.—I did think it possible, when writing my observations page 201 to 203, vol. xxvii., that slips of His Lordship's ship's pen had occasioned his appearing to advance a doctrine, so opposite to all that had been demonstrated by Dr. Smith, Dr. Robison, and a host of other mathematical writers; but his defence of the same in pages 150 to 152, vol. xxviii., precludes any such charitable suppositions in future. The *scientific terms*, or rather the "scientific jargon," of His Lordship, I certainly do not understand, if by that he means, that I am *to receive them*, in opposition to the authorities above quoted, by whom His Lordship was certainly *not* "obliged to use" his new terms, for they have uniformly and consistently used pulses or vibrations for what His Lordship would now for the first time call *beats*; and what he would exclusively call *beatings* they have generally called *beats*, but have sometimes used *beatings* as synonymous therewith.

Before His Lordship took pen in hand on this subject, I well knew that the rate of beating increased along with every increase of the imperfection of a consonance; but His Lordship is the only one I ever heard assert, that it increases "*As the imperfection increases*," which is no more true, than that the *sine* of an angle increases *as* the angle increases, or that gravity increases *as* the distance decreases. His Lordship refers (page 151,) to an example, and attempts to prove, that the triequal quints DA, one an octave above the other, *beat equally quick*: let us therefore see what evidence *numbers* furnish in this case. By referring to my table in page 5, vol. xxx. it will appear, that the two D's vibrate or excite 134·44 and 268·83 complete pulses in the air in one second of time respectively, and the two A's 200·83 and 401·66 pulses respectively, and by using these in the proper theorem for the purpose, we get 1·666 beats per second made by the lower, and 3·333 beats per second by the upper of these tempered or tri-equal quints; the one just *double* of the other, instead of their being *equal* as our noble author has maintained; and thus we see, that no "beating between the two beatings" could in this case happen even *in theory*, and certainly none *in practice* could be expected; for who besides Earl Stanhope ever talked of hearing *beatings*, between two noises which themselves occur but $1\frac{2}{3}$ and $3\frac{1}{3}$

times per second! or not above one-eighth of the rate necessary to constitute continuous or musical sound? In the case of *equally tempered* intervals, situate at the exact distance of *any of the concords* from each other, it is generally true, that no "beating between the beatings" either in theory or practice can happen. Suppose for instance, His Lordship's minor sixth CA which is flattened about $\frac{21}{22}$ parts of a comma (not $\frac{20}{21}$ as printed by mistake p. 195, vol. xxvii.); this in the middle septave beats 22·6335 times per second (or rather, in practice won't beat at all, but produce a continuous third discordant note): if we tune another similar or equally tempered sixth, on a note, a true minor sixth below C the bass of the former one, that is, on His Lordship's first bass E: we shall find, that this will beat just 5-eighths as fast as the above, or 14·1459 times per second, but no "beating between the beatings" will take place, although each are quick enough to produce them, owing to their having the true relation of minor sixth between them, and not because they are *unisons* as His Lordship would contend. Let us, however, abandon the supposition of the tempered sixths having basses that are *exactly* at concordant distances, and tune just a similar minor sixth below C to that which His Lordship has above C, that is, take two of these 6ths *in succession;* then we shall find, the lower note E making 151·79 vibrations per second, and the 6th EC will beat at the rate of 14·3144 times in a second: which not bearing a true concordant relation to the beating of the upper 6th, the sounding of the two together will be found by calculation to occasion a "beating between the beatings" at the rate of 1·3477 per second: thus we see, that a "beating between the beatings" *may* happen to *equally tempered* concords: and the same will indeed *always* happen, in theory at least, to the tempered concords of which His Lordship treats (although His Lordship asserts the contrary); for all his tuning is to be performed by *perfect* intervals except two *successive biequal thirds*, and three *successive triequal quints*, all of which will have such a "beating between the beatings," and of course so acute an observer as His Lordship cannot fail of perceiving them: and will be necessitated to "beat" a retreat, out of the labyrinth of error into which he has with temerity advanced, instead of thinking to "beat" his pretended "facts" and "important musical truths" into me, or any one else who has the least pretensions to mathematical knowledge.

VI.—I have here to complain of the same superficial view of the subject, as His Lordship took when commenting on decimally divided monochords: the object of any *general* notation of musical intervals cannot be to represent the perfect concords, as $\frac{1}{2}$ $\frac{2}{3}$, $\frac{3}{4}$, $\frac{4}{5}$ $\frac{5}{6}$, &c., *more simply* than they are already expressed, but for comparing inconcinnous intervals, such as His Lordship's biequal third for instance, with any other intervals: if we examine the "important musical truths" in *Stereotype* with this view, what do we find, more than that the biequal third has *an approximate* ratio of $\frac{2,371,708,245\,+}{3,000,000,000}$ (page 23)? If we wish to compare this with the triequal quint for instance, whose ratio is stated in the same page, viz. $\frac{3,008,298,850\,+}{3,000,000,000}$, and are desirous to learn their *difference* or the interval remaining after the former is taken from the latter; in vain do we search the records of "musical truths" for the mode of accomplishing this. A novice, misled by the term "difference" in the last column of this page, might think his work easy, and attempt to give us the *difference* of these fractions, already reduced to a *common denominator*, for the purpose; but on discovering that the least interval had the largest numerator, here our tyro's exertions would probably end. One a little more experienced would discover, that it is a *ratio* which is to be deducted, and recollecting his school rule for the division of fractions, would proceed to multiply the denominators and numerators together reciprocally, when after proper reduction, $\frac{2,008,298,850\,+}{2,371,708,245\,+}$ would appear as the ultimate "truth" to be come at.

Now those who have done me the honour, of attending to the *new notation* vol. xxviii. p. 142, would at once discover, that $354\frac{1}{3}\,\Sigma + 7\,\mathrm{f} + 30\frac{2}{3}\,\mathrm{m}$, and $207\frac{1}{2}\,\Sigma + 4\,\mathrm{f} + 18\,\mathrm{m}$, represent the triequal quint and biequal third respectively, and that the difference of these, or $146\frac{5}{6}\,\Sigma + 3\,\mathrm{f} + 12\frac{2}{3}\,\mathrm{m}$, admits of an immediate comparison with all the various intervals in the table in plate V. of the same volume. One simple subtraction would further show it to be, a minor third flattened $14\frac{1}{6}\,\Sigma + 1\frac{1}{3}\,\mathrm{m}$; or $2\frac{1}{6}\,\Sigma + \frac{1}{3}\,\mathrm{m}$ more than the diaschisma or quint-wolf of our noble author: and hundreds of instances might be shown, wherein this notation gives still greater facility to the comparison of intervals with very complex ratios, than it does in the above case; but can His Lordship show a single instance (except the well-known and useful process of reducing large numbers of *pence* to pounds, shillings, and pence, for *some* purposes be considered such,) wherein *his* ingenious notation by scores, dozens, and odd would possess any advantage over decimal arithmetic? analogous to the conversion of simple ratios by the new notation into three elementary ratios (or two in some cases) which I have effected for the general comparisons of intervals? or, can his sapient approvers make out, similar advantages to result from *their* "cubit" and "measuring rod of Ezekiel," for expressing the Lapland degree?

I have not dropped my design of entering at some future time on a comparison of His Lordship's monochord and equal-beating systems, with the systems of other writers, particularly those which His Lordship has in so summary a way condemned, as I originally proposed, by the help of a table of the *temperaments* and *beats* of *every concord* which can arise in each system respectively: and as I am kindly assisted in the labour of these calculations, by a gentleman of more leisure than myself, with whom His Lordship is well acquainted, he has in the mean time the opportunity through him, of himself anticipating my intended comparisons, and of giving, any further support to his systems, which such comparative evidence will warrant.

I am, sir, your obedient humble servant,

JOHN FAREY.

12, Upper Crown Street, Westminster,
March 14, 1809.

LIII. *On*

LIII. *On the Motion of floating Bodies in running Water. By* Peter Barlow, *Esq., of the Royal Military Academy, Woolwich.*

To Mr. Tilloch,—Sir,

In your Magazine for March, I have observed a letter from Mr. G. Orr, wherein he endeavours to account for some particular circumstances in the motion of floating bodies in running waters, that have been noticed by Capt. Burney, and which, it seems, formed the subject of a paper that was read before the Royal Society. In order to accomplish this, Mr. Orr has called in the assistances of the inclined plane, and the doctrine of gravity, or, as he calls them, the *laws of matter and motion*. Mr. Orr will excuse me, at least I hope so, when I inform him that he does not seem to comprehend what those laws are. He is not, perhaps, aware that, in the sense that he attaches to them, he is reviving the old exploded notions of Aristotle, which ever since the time of Galileo have been known to be erroneous:—that celebrated philosopher proved that, by the law of gravity, all bodies, whatever their magnitude and density may be, fall through equal spaces in equal times; and when this is not the case, it arises from those laws being counteracted by some other force .Thus in the case given by Mr. Orr, of two globes of equal magnitudes and different densities rolling down an inclined plane, were they left solely to the action of gravity,—for instance, had the experiment been made *in vacuo,* they would both have descended in the same time; and the reason they do not in ordinary experiments, is because the force of gravity is counteracted by the resistances of the atmosphere; for, both bodies having equal dimensions, they experience equal resistances in their descent, and consequently that body which opposes the greatest force to this resistance, that is, the heaviest body, will descend with the greatest velocity. We see, therefore, that instead of gravity being the cause of the different rates of descent of the two bodies, the circumstance must be attributed to its laws being counteracted. Mr. Orr is not less mistaken in considering this as a parallel case to two bodies floating in running water;

for in the former case the bodies move through a medium which is perfectly at rest; and in the latter the medium itself is in motion, and is the cause of the motion of the bodies.

Having thus, I think, satisfactorily shown that Mr. Orr's *laws of matter and motion* are not sufficient to account for the circumstance alluded to by Capt. Burney, I will venture to offer my own conjectures on this subject;—but this I do with considerable diffidence, and soliciting a correction of any errors that may be discovered therein by any of your ingenious correspondents.

Let us then suppose the case of a beam of timber loaded at one end; and let us conceive that in the first instance the beam moves parallel to itself, or that the velocities of the two ends are equal: then it is evident that the heaviest end will acquire the greatest momentum, and consequently, if the beam should experience any resistance in its motion, that end of it which possesses the greatest momentum will oppose that resistance with the greatest effect, and will thus be thrown foremost; and this will continue to be the case, till its direction coincide with that of the stream.

What the resistance is that the beam experiences, and from what cause it arises, are not difficult to determine. In such a river as the Thames, where the experiments were made, and where the tide is constantly ebbing and flowing, every particle of water cannot be supposed to move with equal velocities; small eddies, contrary currents, and various other circumstances will prevent that uniformity, though perhaps they may be imperceptible to the eye of even an attentive observer. This being the case, if the beam should be struck by any particles of water moving in a contrary direction, or should the beam strike any that are at rest, or moving in the same direction as itself, but with a less velocity, any of these circumstances will oppose a resistance to the motion of the beam, which it has been shown will be more effectively overcome by that end of it which has the greatest momentum. And in a similar manner we may account for the motion of the two barges.

Should these conjectures be thought deserving of a place in your next Number, they are very much at your service.

Yours, &c.,

April 7, 1809. PETER BARLOW.

LIV. *Memoir upon the Formation of the Phosphoric Ether, by Means of a particular Apparatus.* By M. BOULLAY, *Chemist, in Paris,—Read to the First Class of the National Institute the 23d of March,* 1807*.

SCHEELE and Lavoisier had repeatedly tried without success, to transform alcohol into ether, by the action of the phosphoric acid; when at last M. Boudet jun., an apothecary of Paris, published a Memoir upon the subject, in tome xl. of the *Annales de Chimie.* The phænomena which he describes announce a real action between the acid and the alcohol, and manifest several circumstances which generally accompany the process of etherification. According to his own admission, however, the produce he obtained was not very volatile, totally insoluble in water; and although it had a peculiar smell, it did not present the characters of a true ether.

Convinced by various trials that the want of action of the concentrated or even vitreous phosphoric acid upon alcohol, was particularly owing to the difficulty of uniting these two substances, of multiplying and prolonging the contact of their reciprocal molecules, I resumed the experiments, and the hope I entertained of attaining a more satisfactory result was realized by the following experiment:—

To a tubulated retort placed upon a sandbath, I adjusted a bell-glass also tubulated, which communicated by a Welter's tube of safety with a flask filled with limewater. From this flask issued a second tube which proceeded under a bell-glass attached to the hydro-pneumatic apparatus.

I introduced into the retort 500 grammes of pure phosphoric acid, resulting from the combustion of phosphorus by

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* From *Annales de Chimie,* tome lxii. p. 192.

Apparatus for Phosphoric Ether.

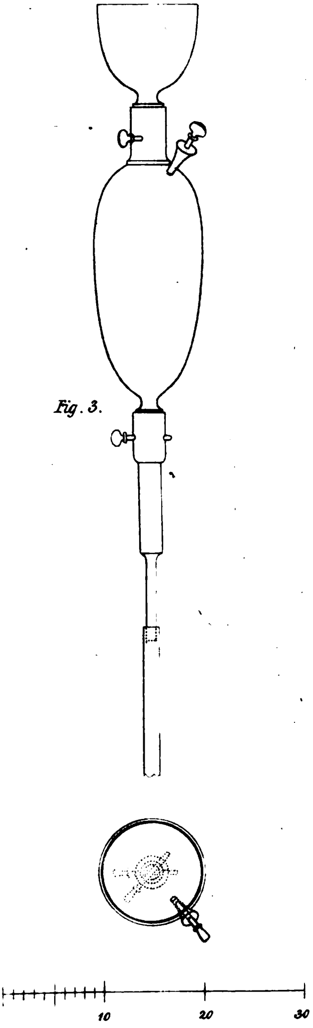

Fig. 3.

Improved Telescopes.

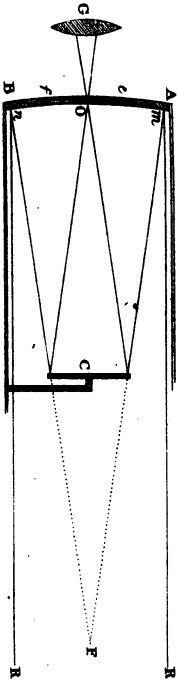

Fig. 1.

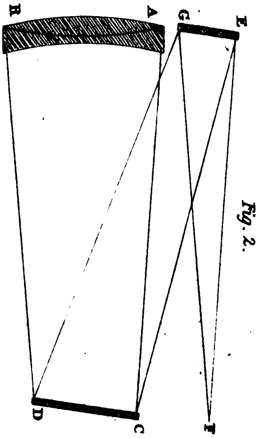

Fig. 2.

S. Porter, sc.

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by the nitric acid, vitrified, redissolved, and reduced by evaporation to the consistence of honey.

I afterwards placed upon the tubulure of the retort an instrument of glass, which may be called the *reservoir*, of an oblong form, open at both extremities, each of which may be made perfectly air-tight by means of a stop-cock. From the lower extremity issued a tube which descended to the bottom of the retort, and entered into the phosphoric acid. The upper extremity surmounted by a funnel, the communication between which and the reservoir might be interrupted, had also a small aperture with a ground stopper, in order to let out the air when it was displaced by a liquid. (See Plate IX. Fig. 3.)

The apparatus being thus arranged, carefully luted, and the first receiver cooled by a mixture of ice and sea salt, a fire was lighted under the retort, increasing it gradually so as to heat the acid to 95 degrees of Reaumur. Five hundred grammes of alcohol at 40 degrees were then introduced into the reservoir, and, by means of the lower stop-cock, drawn drop by drop through the phosphoric acid warm and liquid. The mixture took place with great effervescence: it was coloured black, and abundant streaks immediately covered the arch and neck of the retort.

The fire was kept up, and distillation continued to dryness:

There passed into the bell-glass:

1. One hundred and twenty grammes of alcohol feebly etherized.

2. Two hundred and sixty grammes of a white light liquor, of a lively smell, and much more etherized than the first.

3. Sixty grammes of water saturated with ether, and on which four grammes of a lemon-coloured fluid floated of an empyreumatic smell, very analogous to that which comes after the sulphuric ether, and which is commonly distinguished under the name of sweet oil of wine.

4. Another liquid of a fœtid insupportable smell, reddening turnsole tincture, and uniting with effervescence with the carbonate of potash. This combination evaporated to dryness was a deliquescent scaly salt, perfectly resembling acetate of potash.

Limewater became turbid, but not until after the distillation was over.

Besides the air in the vessels, a gas of a pungent smell was collected, which burned with a white flame, depositing upon the sides of the bell-glass a very abundant charry coating: this was a little ether which had escaped condensation, passed over at the same time with the most etherized liquid product, and a little before the white vapours which announce the presence of oil.

There remained in the retort a vitreous blackish substance, composed of phosphoric acid and a little charcoal.

The two first products united, of the weight of 380 grammes, rectified over dry muriate of lime at a heat of about 50 degrees, furnished about 60 grammes of a liquor having the greatest possible resemblance, in point of smell and taste, to the purest sulphuric ether. Like the latter, it marked 60 degrees in Baumé's areometer, the thermometer being at 10°; it was dissolved in eight or ten parts of cold water, was rapidly evaporated in the air, entered into ebullition at thirty degrees of temperature, dissolved the resins and phosphorus, burned with a whitish flame, leaving a charry residuum, and without any trace of acid having been exhibited by its combustion over the surface of water.

The other products of the rectification were alcohol slightly etherized: this alcohol when again passed, in the manner already shown, through the phosphoric acid used in the experiments, occasioned the formation of a new quantity of ether in every respect similar to the first.

It seems to result from the preceding facts, and from the examination of the above products,

1st, That the phosphoric acid can transform alcohol into a perfect ether by means of the apparatus which I used, and by attending to the precautions already described.

2dly, That the ether which results from the action of the phosphoric acid upon alcohol is, of all the ethers known, that which has the most analogy with sulphuric ether, both with respect to its properties and to the phænomena observed in its preparation.

LV. *Memoirs of the late* Erasmus Darwin, *M. D.*

[Continued from vol. xxxii. p. 336.]

DARWINIANA.

Madness.—In every species of madness there is a peculiar idea either of desire or aversion, which is perpetually excited in the mind with all its connections. In some constitutions this is connected with pleasurable ideas without the exertion of much muscular action, in others it produces violent muscular action to gain or avoid the object of it, in others it is attended with despair and inaction. Mania is the general word for the two former of these, and melancholia for the latter; but the species of them are as numerous as the desires and aversions of mankind.

In the present age the pleasurable insanities are most frequently induced by superstitious hopes of heaven, by sentimental love, and by personal vanity. The furious insanities by pride, anger, revenge, suspicion. And the melancholy ones by fear of poverty, fear of death, and fear of hell; with innumerable others.

> Quicquid agunt homines, votum, timor, ira, voluptas,
> Gaudia, discursus, nostri est farrago libelli.
>
> Juven. i. 85.

This idea, however, which induces madness or melancholy, is generally untrue; that is, the object is a mistaken fact. As when a patient is persuaded he has the itch, or venereal disease, of which he has no symptom, and becomes mad from the pain this idea occasions. So that the object of madness is generally a delirious idea, and thence cannot be conquered by reason; becaue it continues to be excited by painful sensation, which is a stronger stimulus than volition. Most frequently pain of body is the cause of convulsion, which is often however exchanged for madness; and a painful delirious idea is most frequently the cause of madness originally, but sometimes of convulsion. Thus I have seen a young lady become convulsed from a fright, and die in a few days; and a temporary madness frequently terminates the paroxysms of the epilepsia dolorifica, and an insanity of greater permanence is frequently induced by the pains or bruises of parturition.

Where

Where the patient is debilitated a quick pulse sometimes attends insane people, which is nevertheless generally only a symptom of the debility, owing to the too great expenditure of sensorial power; or of the paucity of its production, as in inirritative, or in sensitive inirritated fever.

But nevertheless where the quick pulse is permanent, it shows the presence of fever; and as the madness then generally arises from the disagreeable sensations attending the fever, it is so far a good symptom; because when the fever is cured, or ceases spontaneously, the insanity most frequently vanishes at the same time.

The stimulus of so much volition supports insane people under variety of hardships, and contributes to the cure of diseases from debility, as sometimes occurs towards the end of fevers. And, on the same account, they bear large doses of medicines to procure any operation on them; as emetics, and cathartics, which, before they produce their effect in inverting the motions of the stomach in vomiting, or of the absorbents of the bowels in purging, must first weaken the natural actions of those organs.

From these considerations it appears, that the indications of cure must consist in removing the cause of the pain, whether it arises from a delirious idea, or from a real fact, or from bodily disease; or secondly, if this cannot be done, by relieving the pain in consequence of such idea or disease. The first is sometimes effected by presenting frequently in a day contrary ideas to show the fallacy, or the too great estimation, of the painful ideas. 2dly, By change of place, and thus presenting the stimulus of new objects, as a long journey. 3dly, By producing forgetfulness of the idea, or object, which causes their pain; by removing all things which recall it to their minds; and avoiding all conversation on similar subjects. For I suppose no disease of the mind is so perfectly cured by other means as by forgetfulness.

Secondly, the pain in consequence of the ideas or bodily diseases above described is to be removed, first, by evacuations, as venesection, emetics, and cathartics; and then by large doses of opium, or by the vertigo occasioned by a circulating swing, or by a sea-voyage, which, as they affect the organs of sense as well as evacuate the stomach, may contribute to answer both indications of cure.

Where maniacs are outrageous, there can be no doubt but coercion is necessary; which may be done by means of a strait waistcoat; which disarms them without hurting them; and by tying a handkerchief round their ankles to prevent their escape. In others there can be no doubt, but that confinement retards rather than promotes their cure; which is forwarded by change of ideas in consequence of change of place and of objects, as by travelling or sailing.

The circumstances which render confinement necessary, are: first, if the lunatic is liable to injure others, which must be judged of by the outrage he has already committed. 2dly, If he is likely to injure himself; this also must be judged of by the despondency of his mind, if such exists. 3dly, If he cannot take care of his affairs. Where none of these circumstances exist, there should be no confinement. For though the mistaken idea continues to exist, yet if no actions are produced in consequence of it, the patient cannot be called insane, he can only be termed delirious. If every one, who possesses mistaken ideas, or who puts false estimates on things, was liable to confinement, I know not who of my readers might not tremble at the sight of a madhouse!

The most convenient distribution of insanities will be into general, as mania mutabilis, studium inane, and vigilia; and into partial insanities. These last again may be subdivided into desires and aversions, many of which are succeeded by pleasurable or painful ideas, by fury or dejection, according to the degree or violence of their exertions. Hence the analogy between the insanities of the mind, and the convulsions of the muscles described in the preceding genus, is curiously exact. The convulsions without stupor, are either just sufficient to obliterate the pain which occasions them; or are succeeded by greater pain, as in the convulsio dolorifica. So the exertions in the mania mutabilis are either just sufficient to allay the pain which occasions them, and the patient dwells comparatively in a quiet state; or those exertions excite painful ideas, which are succeeded by furious discourses, or outrageous actions. The studium inane, or reverie, resembles epilepsy, in which there is no sensibility to the stimuli of external objects. Vigilia, or watchfulness, may be compared to the general writhing of the body; which is just a sufficient exertion to relieve the pain which occasions it. Erotomania may be compared to trismus, or other muscular fixed spasm, without much subsequent pain; and mœror to cramp of the muscles of the leg, or other fixed spasm with subsequent pain. All these coincidences contribute to show, that our ideas are motions of the immediate organs of sense obeying the same laws as our muscular motions.

The violence of action accompanying insanity depends much on the education of the person; those who have been proudly educated with unrestrained passions, are liable to greater fury; and those whose education has been humble, to greater despondency. Where the delirious idea, above described, produces pleasurable sensations, as in personal vanity or religious enthusiasm, it is almost a pity to snatch them from their fool's paradise, and reduce them again to the common lot of humanity; lest they should complain of their cure, like the patient described in Horace,

———— Pol! me occidistis, amici,
Non servastis, ait, cui sic extorta voluptas,
Et demptus per vim mentis gratissimus error!

The disposition to insanity, as well as to convulsion, is believed to be hereditary; and in consequence to be induced in those families from slighter causes than in others. Convulsions have been shown to have been most frequently induced by pains owing to defect of stimulus, as the shuddering from cold, and not from pains from excess of stimulus, which are generally succeeded by inflammation. But insanities are on the contrary generally induced by pains from excess of stimulus, as from the too violent actions of our ideas, as in common anger, which is an insanity of short duration; for insanities generally, though not always, arise from pains of the organs of sense; but convulsions generally, though not always, from pains of the membranes or glands. And it has been previously explained, that though the membranes and glands, as the stomach and skin, receive great pain from want of stimulus; yet that the organs of sense, as the eye and ear, receive no pain from defect of stimulus.

Hence it follows, that the constitutions most liable to convulsion, are those which most readily become torpid in some part of the system, that is, which possess less irritability; and that those most liable to insanity, are such as have excess of sensibility; and lastly, that these two circumstances generally exist in the same constitution. These observations explain why epilepsy and insanity frequently succeed or reciprocate with each other, and why inirritable habits, as scrophulous ones, are liable to insanity, of which I have known some instances.

In many cases, however, there is no appearance of the disposition to epilepsy or insanity of the parent being transmitted to the progeny. First, where the insanity has arisen from some violent disappointment, and not from intemperance in the use of spirituous liquors. Secondly, where the parent has acquired the insanity or epilepsy by habits of intoxication after the procreation of his children. Which habits I suppose to be the general cause of the disposition to insanity in this country.

As the disposition to gout, dropsy, epilepsy, and insanity, appears to be produced by the intemperate use of spirituous potation, and is in all of them hereditary; it seems probable, that this disposition gradually increases from generation to generation, in those families which continue for many generations to be intemperate in this respect; till at length these diseases are produced; that is, the irritability of the system gradually is decreased by this powerful stimulus, and the sensibility at the same time increased. This disposition is communicated to the progeny, and becomes still increased, if the same stimulus be continued, and so on by a third and fourth generation; which accounts for the appearance of epilepsy in the children of some families, where it was never known before to have existed, and could not be ascribed to their own intemperance. A parity of reasoning shows, that a few sober generations may gradually in the same manner restore a due degree of irritability to the family, and decrease the excess of sensibility.

From hence it would appear probable, that scrophula and dropsy are diseases from inirritability; but that in epilepsy and insanity an excess of sensibility is added, and the two faulty temperaments are thus conjoined.

Colica flatulenta.—The flatulent colic arises from the too great distention of the bowel by air, and consequent pain. The cause of this disease is the inactivity or want of sufficiently powerful contraction of the coats of the bowel, to carry forwards the gas given up by the fermenting aliment. It is without fever, and generally attended with cold extremities.

It is distinguished, first, from the pain occasioned by the passage of a gall-stone, as that is felt at the pit of the stomach, and this nearer the navel. Secondly, it is distinguished from the colica saturnina, or colic from lead, as that arising from the torpor of the liver, or of some other viscus, is attended with greater coldness, and with an aching pain; whereas the flatulent colic being owing to distention of the muscles of the bowel, the pain is more acute, and the coldness less. Thirdly, it is distinguished from inflammation of the bowels, or ileus, as perpetual vomiting and fever attend this. Fourthly, it is distinguished from cholera, because that is accompanied with both vomiting and diarrhœa. And lastly, from the colica epileptica, or hysteric colic, as that is liable to alternate with convulsion, and sometimes with insanity; and returns by periods.

M.M. Spirit of wine and warm water, one spoonful of each. Opium one grain. Spice. Volatile alcali. Warm fomentation externally. Rhubarb.

The discriminations here merit the utmost praise, and are of the highest use to the practitioner.

Colica saturnina.—Colic from lead. The pain is felt about the navel, is rather of an aching than acute kind at first, which increases after meals, and gradually becomes more permanent and more acute. It terminates in paralysis, frequently of the muscles of the arm, so that the hand hangs down when the arm is extended horizontally. It is not attended with fever, or increase of heat. The seat of the disease is not well ascertained, it probably affects some part of the liver, as a pale bluish countenance and deficiency of bile sometimes attend or succeed it, with consequent anasarca; but it seems to be caused immediately by a torpor of the intestine, whether this be a primary or secondary affection, as appears from the constipation of the bowels, which attends it; and is always produced in consequence of the great stimulus of lead previously used either internally for a length of time, or externally on a large surface.

A delicate young girl, daughter of a dairy farmer; who kept his milk in leaden cisterns, used to wipe off the cream from the edges of the lead with her finger; and frequently, as she was fond of cream, licked it from her finger. She was seized with the saturnine colic, and semi-paralytic wrists, and sunk from general debility.

A feeble woman about 40 years of age sprained her ancle, and bruised her leg and thigh; and applied by ill advice a solution of lead over the whole limb, as a fomentation and poultice for about a fortnight. She was then seized with the colica saturnina, lost the use of her wrists, and gradually sunk under a general debility.

M.M. First opium one or two grains, then a cathartic of senna, jalap, and oil, as soon as the pain is relieved. Oleum ricini. Alum. Oil of almonds. A blister on the navel. Warm bath. The stimulus of the opium, by restoring to the bowel its natural irritability in this case of painful torpor, assists the action of the cathartic.

This disease is generally produced by lead absorbed by the surface—for many practitioners are in the habit of giving sugar of lead, (saccharum Saturni) in spitting of blood, to a considerable extent, as two grains a day, and continued for a fortnight without this disease being produced. The makers of white lead for paint are particularly subject to this disorder, and painters from not keeping the hands clean; and in such case the author of these memoirs has found the highest use in ordering a diet of fat bacon—the corrosive sublimate (hydragyrus muriatus) in a mixture of tincture of bark, an ounce, decoction of bark, six ounces, and two drachms of powdered bark, with two grains of the muriated mercury, of which a table spoonful is to be taken night and

morning

morning—and the bowels are to be first relieved by four grains of calomel, and in half an hour a table-spoonful of castor oil, to be repeated every two hours with fomentations. This practice, so successfully employed, of mercury, as a specific in this disease, appears to be unknown by Dr. Darwin.

[To be continued.]

LVI. *Report on a Manuscript Work of* M. ANDRE, *formerly known under the Name of* P. CHRYSOLOGUE DE GY, *entitled* A Theory of the actual Surface of the Earth. *By MM.* HAUY, LEVIERRE, *and* CUVIER. *Read to the Class of Mathematical and Physical Sciences in the National Institute,* 1807.

[Continued from p. 173.]

ABOUT the commencement of the 18th century, it began to be considered, that one single inundation, however violent it might be, could not produce such immense effects, of which every day developed more and more their extent. It was then necessary to admit of a long series of operations either slow or sudden; and those geologists who still maintained the real existence of a deluge, considered it simply as the last of the revolutions which have contributed to bring the globe into the state we now see it.

This step once taken, hypotheses were no longer limited. In this branch of natural history the systematical method of Descartes was again revived, although Newton appeared to have banished it for ever from the physical sciences. Every one conceived a principle *à priori*, or founded solely on a very small number of partial observations, and employed his skill to accommodate, well or ill, the facts within his knowledge. But, by a fatality hardly conceivable, in the midst of all those efforts, it was almost entirely neglected to extend our knowledge of facts; and when it is remembered that Leibnitz and Buffon were among the philosophers of whom we speak, it will be allowed that it was neither for want of genius nor talents, that so erroneous a method was adopted.

It is thus that the number of systems of geology is so augmented, that there are at present above 80, and that it is necessary to class them in a certain order, only to aid the memory in retaining their leading principles. Yet the example of some of the ablest philosophers, during the last thirty years, has contributed so little to prevent additions to this long list, that we every day see some new systems advanced, and our scientific journals filled with reciprocal attacks and defences of their authors.

How can so many men of talents, replete with science and integrity, be so discordant, and continue such controversies! The reason is very simple; it is, that if one of them were right, neither he nor the others could ascertain it. To discover if a fact is owing to a certain cause, it is necessary to know the nature of the cause and the circumstances of the fact. What, therefore, in the actual state of the sciences, are the authors of geological systems, but persons who seek the causes of facts, before the facts themselves are known? Can we imagine an end more chimerical? Yes: we are ignorant, we do not say merely of the nature and disposition of the interior of the globe, but even of its most exterior crust.

The researches of miners, such as Pallas, Saussure, Deluc, Dolomieu, and the school of Werner, have furnished us with some important general observations, although not yet incontestable, on the primitive mountains: but the secondary ones, which constitute the most embarrassing part of the problem, are scarcely touched; and the principal points, on which necessarily depends what relates to causes, are yet in question. We could cite a multitude of examples; but to be brief, we shall confine ourselves to one or two.

Have organized beings lived in the places where their remains are found, or have they been transported there? Do these beings still live, or have they been wholly or partly destroyed?

Is it not evident that the system of supposed causes must differ as much as black and white, according as these questions are answered in the affirmative or negative? Nevertheless, no person can yet answer them positively; and what is still more singular, no philosopher, I believe, has ever sus-

pected

pected that it would be necessary to be able to answer them before attempting to make a system.

Hence the reason why some will have millions of years for the formation of secondary mountains, while others pretend that about 5000 years ago they were formed in one? All the intermediate parts between these two extremes have had their defenders.

There already exist ten or twelve hypotheses for the partial explanation of the formation of the basin of Paris, yet not one of those who have formed them knows what exists in one small corner of this basin, which contains only a few square toises. At Grignon there are 600 species of unknown shells, besides 40 or 50 that are supposed to be known. This fact is stated by M. de Lamarck, after several years researches. Neither does one of them know that our gypsum contains the bones of 12 or 15 quadrupeds, which neither resemble those seen here or elsewhere; another fact which has only been ascertained after ten years labour.

Judge then what ought to be the explanations coolly imagined in the closet by persons to whom these two little circumstances of the phænomenon were unknown. How then ought a learned society to act, in order to extend and improve so interesting and useful a science, and direct it to real and attainable objects? It ought in this, as in every other science, to encourage by its approbation all those who state positive facts, and preserve the most rigid silence on the systyems which succeed them. In this manner the authors of systems treat the observers or collectors of facts. It is curious to see them all, the moment any discoveries are made by observers, ready to seize them, arrange them according to their own ideas, and convert them into weapons against their adversaries. It appears as if anatomists, zoologists, and mineralogists, were but workmen destined to furnish materials for their fantastical fabrics.

Happily for the example of those who may be tempted to pursue such a course, these castles in the air vanish like apparitions, and the more solid edifice raised on facts and induction begins to appear. The plan, if we may so speak, is already traced; men of judgement at the end of the 18th century have proposed questions; they have already answered some, and have indicated the only means by which the remainder may be resolved. The series of problems is proposed, and nothing but enlightened perseverance is wanted to fill up the outline which constitutes the science.

It is not foreign to the object of our report to present here, as an example, some of the principal objects which appear to us necessary to be profoundly studied, in order to make geology a science of facts, and before attempting, with any hope of success, to answer the grand problem of the causes which have reduced our globe to its actual state. To this end we ought,

1st, To search if the division of great chains in one middle, and two lateral banks or dikes, observed by Pallas, and developed by Deluc, is invariable, and examine, as Ramond has done on the Pyrenees, the causes which sometimes conceal them.

2dly, To examine if there is also any thing certain or uniform in the succession of secondary strata, if such a kind of stone is always below such another, and *vice versa.*

3dly, To operate in a similar manner on the fossils, determine the species which appear the first, and those which are only seen afterwards; discover if these two sorts never accompany each other, if there are any alternations in their appearance; that is, if the first found appear a second time, and if the second have then disappeared.

4th, To compare the fossil with the living species more minutely than has hither been done, and determine if there is any relation between the antiquity of the beds, and the similarity or dissimilarity of fossils with the living beings.

5th, To determine if there is any uniform relation of climate between fossils and those living beings which most resemble them; as for example, if they have migrated from the north to the south, the east to the west, or if there have been mixtures and irradiations.

6th, To determine what fossils have lived where they are now found, what others have been transported there, and if there are, in this respect, uniform rules with regard to the strata, species, or climates.

"7th, To follow minutely their different strata throughout their whole extent, whatever may be their doublings, inclinations, ruptures, and slopings; and also to determine what countries belong to one and the same formation, and what others have been formed separately.

8th, To follow the horizontal beds and those which are inclined in one or different ways, to determine if there is any relation between the greater or less constancy in their horizontal position, antiquity, or nature.

9th, To determine the valleys in which the re-entering and saliant angles correspond, and those in which they do not; also those in which the strata are the same on both sides, and those in which they differ, in order to discover if there is any relation between these two circumstances, and if each of them taken apart has any analogy with the nature and antiquity of the strata composing the heights which limit the valleys.

All these points are necessary to its elucidation, if we wish to make geology a body of doctrine or a real science, independent of every desire which we may have to find an explanation of facts. We dare affirm that there is not one of those points on which any thing absolutely certain is yet known, every thing which has hitherto been advanced being more or less vague. The greatest part of those who have treated of such subjects, have considered them rather as they answered their system, than according to impartial observations.

The fossils alone, singly considered, may yet furnish matter for the study of 30 years to several industrious philosophers; and their connections with their strata will still require as many more years of travel, of boring, and other arduous researches.

What service would not a society such as ours render to the natural sciences, if it succeeded in directing to these long, laborious, but determinate researches, those persons with an ardent desire of knowledge, who are now likely to be led, by the contagious example of many men of merit, to the adoption of systems so easily created and so flattering to vanity! The work of M. André, examined according to these principles, presents two distinct parts, only one of which falls within the province of this class. It is that in which this philosopher relates his observations during his travels.

Faithful to the laws of the religious order to which he belongs, M. André has traversed on foot numerous and extensive routes: he travelled as an enlightened observer, noted with care the elevations and cavities of the earth; the nature of stones, and their relative position to each other and to the horizon. He has taken for a model the geologist who first merited this honour, the celebrated Saussure; that is to say, he has described in a precise manner all the objects which struck him on his route; in the order in which they occurred.

A chain of mountains traversed and described with so much care, forms the subject of a general view which M. André has not failed to trace. It is thus that he exhibits the part of the Alps which he has seen, and which comprehends the space between St. Gothard and St. Bernard. He afterwards passed to the Jura, a secondary ridge very different from the Alps, which he examined from the fall of the Rhone to the Rhine, that is, nearly its whole length. The Vosges are the third ridge, a part of which was examined from Epinal to Giromaguy. He describes the bank of separation which on the one side throws the water to the ocean, and on the other to the Mediterranean; he likewise passed from the summit of Salius almost to Cluni; observed and described a part of the plains which unite the Alps to the Jura, and those which, commencing at the Saone, follow the course of the Rhine to Strasburgh.

Although M. André, throughout the whole of the first part of his work, frequently alludes to opinions which he endeavours to prove in the second, it is not the less valuable for a great number of interesting facts which he details, and which are independent of all system. Such in the first place are the *circi*, or circular spaces sunk between high sheltered rocks, which he frequently observed in the Alps. Such, also, are his remarks on certain isolated pyramids, formed of divers layers or strata, the contiguous parts of which must necessarily have been carried off by some cause, although no vestiges of them can now be found at the feet of these pyramids.

In Vallais, M. André describes many steep banks and erosions of the water, which escaped Saussure, who had seen only the lower part of the country, and that during no more than two days. Nevertheless he also shows that this great valley, so far from having saliant and re-entering angles corresponding on two sides, enlarges and contracts alternately even to five times. In general the article Vallais is one of the most complete in this work, M. André having traversed it several times and by different routes. He points out, in several places of the Alps, examples of schistose layers twisted or bent in many directions, which it would be difficult to reconcile with the common theories. In general, however, he appears very little favourable to the idea of the displacing of strata.

His description of *Mont Blanc* is precise and perspicuous, and will be read with interest even after that of Saussure, to whose veracity and accuracy he renders perfect justice. With the same care he has described *St. Gothard* and its environs. He remarks that the highest ridges are not in the central chain; a similar fact occurs in the Vosges. M. Ramond discovered the same thing in the Pyrenees.

In his description of *Jura* he carefully distinguishes the compact calcareous rock without petrifactions, which forms the central parts of the chain, from the calcareous congregation of shells which compose the lateral and less elevated parts. He observed rolled pebbles, and large calcareous stones worn round by moving, like the masses of granite in the Alps; the latter also were discovered in Jura, although not believed to exist by Saussure, who had not sufficiently examined it. M. André likewise speaks of numerous caverns and hollows in this chain. He describes its *glaciers*, particularly the lime glacier five leagues from Besançon. Of this he gives the temperature taken at different periods of the year, to show that it is far from being the reverse of the external air, as some persons have alleged.

His comparison of the Alps, of Jura, and of Vosges, is curious; in the Alps there are longitudinal and transversal valleys; in Jura these are almost all longitudinal; in the Vosges almost all are oblique. We know that the Pyrenees have a fourth structure, and that the valleys there are very nearly all perpendicular. The Vosges are singular for the quantity of *gres* and of puddingstone, which cover their isolated summits, and which appear to be the vestiges of an immense platform.

From these details it will appear that M. André has carefully observed the countries over which he has travelled, that the facts which his work contains may be very valuable to positive geology, at least in what relates to the mineral masses; and although he was not particularly occupied with the fossils, we consider that he must take, in this respect, a distinguished rank among observing geologists.

To his own descriptions of the countries which he visited, he has added several extracted from the best authors, such as Saussure, Deluc, Dolomieu, Ramond, and Patrin, on those which he has not seen. Hence the author infers, that there must be a great analogy between distant regions, and that the theories applicable to these countries must be nearly so to the whole earth. At the conclusion he says something of fossils, but solely after other naturalists. Having thus established his data with great care, either from his own observations or from the most respectable authorities, M. André proceeds to the consequences which he thinks must result from those different facts. After what we have said at the commencement of our report, it will not be expected that we should pronounce judgement on this part of the work; but we shall not abstain from giving an idea of it.

He thinks that the actual arrangement of the surface of the earth has not existed from a very remote epoch, and he endeavours to prove it, like MM. Deluc and Dolomieu, by the progress of depositions (*éboulemens*), and by that of decomposition and formation of soil (*attérissemens*). He likewise thinks that this arrangement is totally owing to a cause unique, general, uniform, violent, and prompt; and appears to attribute to this cause even the transport of foreign

fossils.

fossils. He attempts to prove that neither volcanoes, earthquakes, rivers, nor currents, could possibly arrange the surface of the earth as it is in the present day.

These ideas have also been entertained by several celebrated naturalists, especially when restricted to the last change experienced by the earth. Your committee (*commissaires*) even feel themselves able personally to adopt them in part, although they well know that the reasons which determine them cannot have the same influence on all the world. Yet, for the reasons which they have before stated, they do not wish to engage the Class to pronounce on such subjects. But they do not hesitate to propose, that the Class should testify to M. André the esteem which it owes to his laborious researches, and to the enlightened zeal which induces him to continue his useful labours at so advanced an age. They do not doubt that the work of this respectable philosopher will be received by naturalists as a collection so rich in interesting facts ought to be.

LVII. *Observations upon Subterraneous Heat, made in the Mines of Poullaouen, and of Huelgoat, in Britany, in France.* By J. F. DAUBUISSON*.

THERE are few questions in physics, respecting which it is more necessary to be in possession of positive and well established facts, than the temperature of the interior of the globe, taken at depths which it is in our power to visit. I have already published some facts on this subject with respect to the mines of Saxony, and now proceed to detail some others resulting from observations made last summer (1806) in the mines of Poullaouen, and of Huelgoat, in Britany. The habits to which I have been accustomed of examining these subjects, added to my knowledge of the country, having enabled me to choose, with some discernment, the points where I wished to ascertain the temperature, I hope that the facts I am about to relate will not be uninteresting to those who are occupied with the physics of the

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[*] From the *Journal des Mines*, vol. xxi. p. 119.

the terrestrial globe. The thermometer I used was a mercurial one, and graduated into twenty-four parts, from the freezing point to that of boiling water. It was inclosed in a glass tube. I ascertained by experiment, that when it indicated a certain degree of heat, and when it was removed about twelve degrees therefrom, three or four minutes were requisite if it was dipped in water at the freezing temperature, and eleven or twelve minutes when held in the air. According to these data, at all times when I wished to take the temperature of a mass of water in mines, I plunged the thermometer entirely into it and kept it there five minutes: when a mass of air was to be examined, I held the thermometer a quarter of an hour. All these observations were reduced to the centigrade thermometer. However great the care and patience I bestowed, I could never answer precisely within a quarter of a degree.

Observations made at Poullaouen.

I shall begin by describing the position of the place.

The mine of Poullaouen is situated in 48° 17′ 49″ of latitude, and 5° 55′ 57″ longitude west from Paris: its orifice (St. George's pits) is 106 metres above the level of the sea. It is four myriametres from the north extremity of Britany, and six from the south and east extremities. The country in which it is situated forms part of the tongue of land which, in the form of a roof, the ridge of which is 260 metres above the level of the sea, advances into the ocean, and constitutes the country called Britany. The district in which the mine is situated is about 150 metres above the level of the sea: this country is broken up in every direction by valleys; one of them resembles an almost circular basin about a thousand metres in diameter, and it is under the soil of this basin (which is 106 metres above the level of the sea) that the mine of Poullaouen is wrought.

According to the law followed by the heat of the equator at the pole, the mean temperature of the surface of the earth at Poullaouen ought to be 12·4°*. The elevation of the ground requires nearly a degree of diminution; so that we may estimate the mean temperature as 11·5°.

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* I have been led both from theory and observation to use an extremely simple expression describing the thermometrical temperature of any place, the latitude of which is known. This expression is, 30·7° Cos. $^{2\cdot25}$ latitude; or with a sufficient exactitude in the temperate zone, 28° Cos. ^{2} latitude.

My observations were made on the 5th of September 1806. During the whole of the day the sky was serene and cloudless. The temperature taken in the shade at mid-day was 19°.

In detailing the rest of my observations, I shall lay down the position of the points at which they were made, as well as what appeared to me to influence the temperature. By the side of each thermometrical expression I shall give the depth below the surface of the point to which it refers:

1st, In the first gallery, called *the level of 50 feet,* near the pit by which we descend, in a place where there is but a feeble current of air, a little water which was upon the ground indicated,—Temperature 13·1°. Depth 16^{m}.

2d, In the gallery of St. George, under the intersection of three branches of the ridge, in a kind of *cul de sac,* far distant from the place occupied by the miners, where there was no current of air, but from the upper part of it a great quantity of water filtered. This water gave,—Temperature 11·9°. Depth 39^{m}.

3d, The water of filtration which fell into this gallery (St. George's) indicated, on being brought to the mouth of the pit,—Temperature 12·1°. Depth 39^{m}.

4th, Thirty-six metres lower down, at the level of la Boullaye, towards the extremity of a long gallery, where there is neither a current of air nor a single workman; in the water I found,—Temperature 11·9°. Depth 75^{m}.

5th, At the very bottom of St. George's pit, in the hole wherein the waters collect which have fallen from above; the water indicated,—Temperature 14·2°. Depth 142^{m}.

6th, The air above this water,—Temperature 15°. Depth 141^{m}.

7th, In the hole at the bottom of the pit St. Barbe, (at the other extremity of the mine,) in the water I found,—Temperature 13·5°. Depth 150^{m}.

8th, In the air above this water,—Temperature 14·4°. Depth 150^{m}.

9th, The water of the old excavations adjoining,—Temperature 13·3°.

N.B. The waters coming from the filtrations (which principally take place in the upper parts of the ancient works) are cold; and as they form the greater part of those which flow into the pit or well of St. Barbe, they are the cause of the little heat presented by those which exist there.

10th, In an excavation not far distant from the bottom of the well of St. Barbe (called the furnace gallery), the sides of which are almost completely covered with radiated pyrites, partly efflorescent, the thermometer left for more than a quarter of an hour in a small hollow made in the midst of the pyrites, and which contain a good deal of white sulphate,—in this case the thermometer stood at,—Temperature 14·6°. Depth 140^{m}.

11th, When afterwards plunged into a small hole whence a very strong spring issued, it also stood at,—Temperature 14·6°. Depth 140^{m}.

Consequences.—1st, Observations 2, 3, and 4, prove incontestably that the heat of the rock in the upper parts of the mine is 12°, as the waters which indicated it filtered through the rock; and we find that this temperature does not sensibly differ from that pointed out by theory. If the first Observation gave a greater heat, it is because it was made in a place through which air from without continually passes; and this air was warm, the experiments having been made at the end of summer.

2d, Observations 5 and 6 also show that the temperature of the lower parts of the mine is more considerable than that of the upper parts. If in deep places the air appears to be warmer than the water, it is probably because it has preserved a part of the heat which it had upon entering the mine. I have already assigned the reason which accounts for our having in Observations 7, 8, 9, a less heat than might be expected from the depth.

3d, Experiments 10 and 11 show that there are cases in which the presence of pyrites does not produce heat: the heat indicated in these cases cannot depend upon that cause: in the pit of St. George there is no pyrites, and the temperature is the same.

Thus, if we abstract every extraordinary cause, the Observations I have related appear to me to indicate that, at the depth of 150 metres, the temperature is at Pouallouen 3° or 4° higher than at the surface of the ground.

Observations made at Huelgoat.

The mine of Huelgoat is situated at 48° 18′ 17″ latitude, and 6° 1′ 46″ of west longitude: its orifice (the mouth of the pit) is 173 metres above the level of the sea. It is situated upon a broad hill, which separates two valleys, the depth of which is from 80 to 90 metres.

From what we have said as to the latitude and elevation, we may conclude that the mean temperature is 11°.

The rock like that at Poullaouen is an argillaceous schist, and also contains several strata of aluminous schist.

The following are the Observations made by me on the 5th of September, being on the same day with those made at Poullaouen.

1st, In a gallery about five metres below the one by which the workmen generally enter the mine, which has no other orifice but one, and which no person has entered for many years, where there is no current of air, the thermometer placed at its northern extremity marked in about 20 minutes,—Temperature 11°.

After having descended the pit called the Miners' pit, I entered another pit which adjoined a gallery absolutely without any communication with the rest of the mine, and in which there was consequently no current of air.

2d, The thermometer, when plunged into a little stagnant water upon the ground, rose to,—Temperature 12·2°. Depth 70^{m}.

3d, I reascended to the first gallery, and in the water of a gutter, in a place through which a current of air passed, the thermometer marked,—Temperature 13·7°. Depth 60^{m}.

I then proceeded southward, to the spot where they were then working.

4th, In the second gallery, a little way from the pit by which the produce of the mines is extracted, in a place where there was a continual and strong current of air, a little stagnant water marked,—Temperature 15°. Depth 80^{m}.

5th,

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5th, In the fifth gallery, the thermometer, plunged into a water-tank near the great pit, rose to,—Temperature 17°, Depth 160m.

6th, At the extremity of the gallery No. 9½, a great quantity of water is seen to issue from the rock slightly vitriolic; the thermometer, when held a quarter of an hour in the midst of the jet, constantly marked,—Temperature 19·7°. Depth 230m.

7th, When held in the air on one side, it also marked,—Temperature 19·7°. Depth 230m.

8th, About 60 paces nearer the mouth of the pits, the water of the stream formed from the above jet stood also at,—Temperature 19·7°. Depth 230m.

The bottom of the mine was under water which was 16 metres deep. I descended through a small pit, a short way from the great one, to the level of the subterraneous lake.

9th, The thermometer, when kept for a quarter of an hour upon a plank floating in the water, marked,—Temperature 18·8°. Depth 238m.

10th, When plunged in water it also indicated,—Temperature 18·8°. Depth 238m.

All the water which flowed into this southern part of the mine proceeded to the subterraneous lake from which it was pumped up.

11th, The temperature of the water poured into the gallery No. 7, from the pump, was,—Temperature 19·4°. Depth 180m.

Proceeding along this gallery the water flowed into another pit in the northern part of the mine.

12th, Here it mingled with a small quantity of water, the temperature of which was 15°. Depth 120m.

13th, And when the whole together were poured, by means of pumps, into the uppermost gallery, they marked,—Temperature 18·4°.

We have here two classes of observations, which must be kept distinct; namely, those made in the northern, and in the southern part of the mine.

The former, in my opinion, indicate the natural temperature of the soil. No. 1, being made 20 or 30 metres below the surface of the ground, ought to be regarded as giving the real degree of heat of the surface of the country in general. I see no cause which could alter the temperature naturally proper for this place, which is far distant from any working places: one thing is certain, that it continues the same during the whole year; and the result is precisely the same as pointed out by theory. Observations 2 and 3 also show that this temperature increases in proportion as we descend. The current of air in the first gallery accounts for the trifling excess of heat we remark there in proportion to the depth.

As to the temperature of the Observations made in the southern part of the mine, it is visibly influenced by an extraneous cause; namely, by the vitriolic water which flows from the south. On digging a new pit 100 metres distant from the south part of the present workings, they have cut through beds of an aluminous schistus, which has a very strong styptic taste. By the help of a microscope we discover in it a multitude of pyritous points, which, by their decomposition and their action upon the schistus, have probably produced a disengagement of caloric, which must have heated the water passing through these beds. The latter not being very deep, communicate with the atmosphere by some fissures, while decompositions and disengagements must have been effectuated in the interior of the earth.

However this may be, it seems certain that it is by passing through these beds that the water must have acquired a heat of 20°, a heat far superior to that which agrees with the depth at which it is found.

I shall also here observe, that if we ascribe this heat to the pyrites, they produce it by their action upon the schistus. In the observations made at Poullaouen, we have seen pyrites in a considerable quantity occasion no particular increase of heat. I shall repeat here what I have said in another place: I have seen workings of pyrites, and I have not found the heat sensibly stronger than in other mines: thus I am led to think that the pyrites in a mass, at least those not radiated, produce no subterraneous heat: but those which are disseminated in minute particles in a body upon which the sulphuric acid can have an action, act differently when there is an accession of atmospheric air. I have remarked in another Memoir, that it is not the coal which contains most pyrites, which gives in the inside of the mines the inflammable gas known by the name of fire-damp, but rather the coal which contains little or none visible to the naked eye, and in which the sulphuret of iron probably exists in particles not discernible.

LVIII. *Method of ascertaining the Value of Growing Timber Trees, at different and distant Periods of Time.* By Mr. CHARLES WAISTELL, *of High Holborn* *.

SIR,

CONCEIVING that the Tables contained in the annexed papers will afford useful information to growers of timber, and tend to encourage the growth of it in these kingdoms, and thereby promote the views of the Society of Arts, &c. I trust you will have the goodness to lay them before the Society, as I have formed them with great attention.

Having last autumn viewed some plantations made under my direction about thirty years ago, I found the value of one of them much to exceed my expectation. I became therefore desirous to devise some means of estimating what its value might probably be at different future periods. I was thus led to construct the first of these tables, and on the completion of this, other tables seemed necessary; and I was thus progressively led on to the construction of the whole. For this purpose I searched in various authors for the measure of trees, in girt and height, at different ages, and obtained similar information among my acquaintance. Hence I collected, that the increase in the circumference of trees is generally from about one to two inches annually, and from twelve to eighteen inches the annual increase in height. Some fall a little short, and some exceed those measures.

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* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1808.——The gold medal of the Society was voted to Mr. Waistell for this communication.

I shall now briefly notice a few of the advantages to be derived from the first Table.

1st, The first Table shows every fourth year, from twelve years old to a hundred, the rates per cent. per annum at which all trees increase, whether they grow fast or slow, provided their rate of growth does not vary. This table may be the means of saving young thriving woods from being cut down, by showing how great a loss is sustained by felling timber prematurely *.

2d, And it may be the means of bringing old trees to market, by showing the smallness of the interest they pay for the money they are worth, after they are 80 or 100 years old.

But this table shows the interest which they pay, only, whilst the trees continue growing at their usual rate. In case they fall short only a little of their usual increase in girt, this considerably diminishes the rate per cent. per annum of their increase. And trees do decrease in their rate of growth before they appear to do so †. A pale and mossy bark are certain indications of it.

3d, The first Table may also assist the valuer of such timber as is not to be cut down, but to continue growing, by enabling him to estimate its present value more accurately

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* "A wood, near West Ward, in Cumberland, of more than 200 acres, was felling in 1794, it was little more than 30 years old. The whole was cut away without leaving any to stand." See *Miller's Gardener's Dictionary, last edition, under the Head of Woods.*

At 30 years old timber pays 10 per cent. for standing, and probably this wood might have paid 7 per cent. per annum on an average for the next 30 years.

† In Mr. Pringle's Agricultural Report of Westmoreland is a paper of the Bishop of Landaff's, stating, "That a very fine oak, of 82 years growth, measured in circumference at six feet from the ground, on the 27th of October 1792, 107 inches, and on the same day of the same month in 1793, it measured 108 inches." He then states the interest it paid to be only about 2 per cent., and says this tree was a singularly thriving one. It is evident that, with all this appearance of thriving, it was on the decline. For if we divide 108, its inches in circumference, by 82, its age, we find its average annual increase had been one inch and a third. Its falling off to one inch reduced the rate per cent. of increase one-fourth.

than is usually done, especially when it is increasing after a high rate per cent. per annum*.

The second Table shows the rate per cent. to be the same as in the first Table, though the annual increase is more both in height and circumference.

The third Table is calculated to show the number of trees that will stand on an acre of ground, at the distance of one-fifth of their height, (which distance is recommended by Mr. Salmon, in a paper in the Society's 24th volume,) and the number of feet the trees will contain, both those to be cut out, and those to be left standing, at the end of every four years, from 16 to 64 years old, supposing they increase 12 inches in height and 1 in circumference annually. This distance may suit fir trees, but will be too near for oaks.

The fourth and fifth Tables show the same particulars when the trees grow at greater rates.

The sixth Table is calculated to show the same particulars when the trees are constantly thinned out every four years, so as to leave them at the distance of one-fourth of their height. According to this table there will be 48 trees left on an acre when they are 120 years old; and it seems generally agreed, that from 40 to 50 full-grown oak trees are as many as have sufficient room to sand on an acre.

The seventh Table shows the same particulars respecting trees which increase 15 inches in height and 1½ inch in circumference annually.

The eighth Table shows the same particulars respecting trees which increase 18 inches in height, and two inches in circumference annually.

The ninth Table shows the same particulars as Table 6, till the trees are 28 feet high, after which the distance is increased from one-fourth to one-third of their height.

The 10th, 11th, and 12th Tables show the annual increase in boles of 24, 32, and 40 feet long, and the difference of their increase at the same ages.

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* A fir wood of more than 30 acres, and about 30 years old, was lately valued to be sold with an estate, by several eminent wood-valuers, without taking into consideration its rate of increase. It was then increasing after the rate of 10 per cent. per annum, and probably would increase after the rate of 8 per cent. on an average for the next 20 years.

To these Tables succeed comparative statements, showing the number of feet contained in boles of different lengths, when the trees are 60 years old; by which it appears, that, if cut down at that age, the longest boles are not the most profitable to the growers of timber.

And I have added the valuation of the plantations before alluded to, with remarks on them.

Having finished my introductory remarks, I conclude, and am, Sir, your very humble servant,

CHARLES WAISTELL.

Tables respecting the Growth of Timber.

Calculations, showing every fourth year, from 12 to 100, the progressive annual increase in the growth of trees, and gradual decrease in the rate per cent. per annum, that the annual increase bears to the whole tree.

The whole height of the trees is taken to the top of the leading shoot, and the girt in the middle; but no account is taken of the lateral branches.

If trees increase 12 inches in height and 1 in circumference annually, their increase will be as undermentioned, viz.

TABLE I.

Years old & ft. high	Girt.	Contents.			Years old & ft. high	Girt.	Contents.				One year's increase.				Rate per cent. of increase.
	inch.	ft.	in.	pts.		inch	ft.	in	pt.	sds.	ft.	in.	pt.	sds.	
12	1½	0	2	3	13	1⅝	0	2	10	3	0	0	7	3	26·8
16	2	0	5	4	17	2⅛	0	6	4	9	0	1	0	9	19·9
20	2½	0	10	5	21	2⅝	1	0	0	8	0	1	7	8	15·7
24	3	1	6	0	25	3⅛	1	8	4	1	0	2	4	1	13·
28	3½	2	4	7	29	3⅝	2	7	9	1	0	3	2	0	11·
32	4	3	6	8	33	4⅛	3	10	9	6	0	4	1	6	9·67
36	4½	5	0	9	37	4⅝	5	5	11	5	0	5	2	5	8·5
40	5	6	11	4	41	5⅛	7	5	8	10	0	6	4	10	7·6
44	5½	9	2	11	45	5⅝	9	10	7	9	0	7	8	9	6·96
48	6	12	0	0	49	6⅛	12	9	2	3	0	9	2	3	6·38
52	6½	15	3	0	53	6⅝	16	1	10	2	0	10	10	2	5·9
56	7	19	0	8	57	7⅛	20	1	1	7	1	0	5	7	5·4
60	7½	23	5	2	61	7⅝	24	7	6	6	1	2	4	6	5·1
64	8	28	5	4	65	8⅛	29	9	7	0	1	4	3	0	4·76
68	8½	34	1	4	69	8⅝	35	7	8	11	1	6	4	11	4·49
72	9	40	6	0	73	9⅛	42	2	6	4	1	8	6	4	4·2
76	9½	47	7	6	77	9⅝	49	6	5	2	1	10	11	2	3·98
80	10	55	6	8	81	10⅛	57	7	11	9	2	1	3	9	3·79
84	10½	64	3	8	85	10⅝	66	7	7	8	2	3	11	8	3·6
88	11	73	10	4	89	11⅛	76	5	11	1	2	7	7	1	3·5
92	11½	84	5	9	93	11⅝	87	3	4	0	2	9	7	0	3·3
96	12	96	0	0	97	12⅛	99	0	4	6	3	0	4	6	3·15
100	12½	108	6	0	101	12⅝	111	9	6	8	3	3	6	8	3·

In Table X. of the increase of a bole of 24 feet in height, of a tree growing at the above-mentioned rate, it will be observed, that the contents at 24 years of age are the same, and at 64 years nearly the same as in the above Table, but the contents of the bole at all the intermediate periods exceed the above. And a 40 feet bole exceeds the above contents from 44 years to 100, as may be seen in Table XII. For these reasons chiefly I did not think it necessary to take into consideration the decrease in height that takes place in trees at different ages, according to the kind of tree and quality of the soil.

The increase per cent. per annum is the same as the above in all trees at the same age, whether they have grown faster or slower, provided their increase in height and thickness annually has not varied on an average. The progress of trees is sometimes greatly retarded by insects destroying their leaves, by unfavourable seasons, and by their roots penetrating into noxious strata. But these accidents cannot enter into calculations.

Calculations, showing every fourth year from 12 to 64, the progressive annual increase in the growth of trees, and the gradual decrease in the rate per cent. per annum that the annual increase bears to the whole tree.

The whole height of the trees is taken to the top of the leading shoot, and the girt in the middle; but no account is taken of the lateral branches.

If trees increase eighteen inches in height, and two inches in circumference, annually, their increase will be as undermentioned, viz.

TABLE II.

Age of Trees	Height	Girt.	Contents.			Age of Trees.	Height	Girt.	Contents.				One year's increase.				Rate per cent. of increase.
	feet.	in.	ft.	in.	pt.		feet.	inch.	ft.	in.	pt.	sd	ft.	in.	pt.	sd.	
12	18	3	1	1	6	13	19½	3¼	1	5	1	0	0	3	7	0	26·5
16	24	4	2	8	0	17	25½	4¼	3	2	4	0	0	6	4	0	19·8
20	30	5	5	2	6	21	31½	5¼	6	0	3	6	0	9	9	6	15·6
24	36	6	9	0	0	25	37½	6¼	10	2	0	6	1	2	0	6	13·
28	42	7	14	3	6	29	43½	7¼	15	10	6	0	1	7	0	0	11·
32	48	8	21	4	0	33	49½	8¼	23	4	8	0	2	0	8	0	9 6
36	54	9	30	4	6	37	55½	9¼	32	11	7	6	2	7	1	6	8·5
40	60	10	41	8	0	41	61½	10¼	44	10	3	6	3	2	3	6	7 6
44	66	11	55	5	6	45	67½	11¼	59	3	10	0	3	10	4	0	6·9
48	72	12	72	0	0	49	73½	12¼	76	7	1	0	4	7	1	0	6 3
52	78	13	91	6	6	53	79½	13¼	96	10	11	6	5	4	5	6	5·8
56	84	14	114	4	0	57	85½	14¼	120	6	8	6	6	2	8	6	5·4
60	90	15	140	7	6	61	91½	15¼	147	9	2	0	7	1	8	0	5·
64	96	16	170	8	0	65	97½	16¼	178	9	4	0	8	1	4	0	4·7

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Explanation of the Construction of Tables I. *and* II.

To render the preceding tables easy to be understood by persons not accustomed to calculations, I will state the process of the operations in the first line of Table II.

The height of the tree at 12 years of age is supposed to be 18 feet to the top of its leading shoot, aud 24 inches in circumference at the ground, consequently, at half the height, the circumference is 12 inches,—one-fourth of this, being three inches, is called the girt. The girt being squared and multiplied into the height, gives one foot one inch and six parts for its contents. At 13 years old the tree will be 19½ feet high, 26 inches in circumference at the ground, and 13 inches at half the height; one-fourth of 13 gives 3¼ inches for the girt. This squared and multiplied into the height, gives one foot five inches and one part for the contents. Deduct from this the contents of the tree at 12 years of age, and there remains three inches and seven parts, which is the increase in the 13th year. Then reduce the contents of the tree when 12 years old, and the increase in the 13th year, each into parts, dividing the former by the latter, and the quotient will be 3·76; by this number divide 100, and the quotient is 26·5, which is the rate per cent. of increase made in the thirteenth year; consequently whatever the tree might be worth when 12 years old, it will, at the end of the 13th year, be improved in value after the rate of 26*l.* 10*s.* per cent., or in other words, that will be the interest it will have paid that year for the money the tree was worth the preceding year.

At every succeeding period, both in this Table and Table I., the like process is gone through.

[To be continued.]

LIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 13.—Earl of Morton in the chair. A paper by the Rev. Mr. M^c^ Gregor, on native arseniate of copper, was read. The existence of this substance in nature has long been held problematical, and its discovery in a mine between

50 and 60 fathoms below the surface of the earth, in Cornwall, is an additional stimulus to pursue our researches. This mineral is of a pale yellow colour; two specimens of it were analysed by Mr. M^{c}Gregor, one of which contained 69.—of arsenic acid, and 26.—copper; the other 72.—acid, and 28.—copper. Some muriate of iron and silica were also found, but they are deemed not essential to the mineral.

April 20.—The President in the chair. Dr. Chisholm laid before the Society some particulars respecting a race of pygmies, said to exist as a nation in the centre of the island of Madagascar. A M. Baudin, who had visited that island and spent 50 days among them, and who was in the French West Indies, had one of these beings preserved; it was a man about 33 years of age, measuring only 32 inches, but perfectly proportionate in all his parts. A child of a year old was also preserved in spirits, and measured one foot. These people are represented as being much fairer than the other natives, and of a bright copper colour; they are also said to be very ingenious, to be expert with bows and arrows, or javelins; and to be hospitable, humane, and generous. One account states them to have long hair, and another short and woolly. They are also very numerous, M. Baudin having seen above 8000 in one town. The women are said to have little breasts and almost no milk, so that the children are fed with that of cows. Dr. Chisholm, who personally inspected and measured these preserved bodies, concludes, that a pygmy race should no longer be considered as fabulous, and that such has now been discovered in Madagascar. Some other French voyagers have likewise mentioned the existence of these singular people.

SOCIETY OF ANTIQUARIES.

Monday, April 24, (St. George's day falling on a Sunday,) the Society of Antiquaries met at their apartments in Somerset-place, in pursuance of their statutes and charter of incorporation, to elect a president, council, and officers of the Society for the ensuing year: whereupon—

The Most Noble George Marquis of Townshend and Earl of Leicester; F. A. Barnard, Esq.; W. Bray, Esq.; Nich.

Carlisle, Esq.; F. Douce, Esq.; Sir H. C. Englefield, Bart.; A. Hamilton, D.D.; S. Lysons, Esq.; C. Ord, Esq.; M. Raper, Esq.; J. Windham, Esq., (eleven of the Council,) were rechosen of the New Council; and—

George Earl of Aberdeen; J. Caley, Esq.; W. Hamilton, Jun. Esq.; A. B. Lambert, Esq.; Charles Lord Bishop of Oxford; R. Pearson, M.D.; T. B. Richards, Esq.; Sir J. T. Stanley, Bart.; J. Symmons, Esq.; H. N. Willis, Esq., (ten of the other members of the Society,) were chosen of the New Council: and they were severally declared to be the Council for the year ensuing. And on a report made to the officers of the Society, it appeared, that—

The Most Noble George Marquis of Townshend and Earl of Leicester was elected President; William Bray, Esq., Treasurer; William Hamilton, Jun. Esq., Director; Rev. Thomas William Wrighte, A.M., Secretary; and Nicholas Carlisle, Esq., Secretary for the year ensuing.

The Society afterwards dined together at the Crown and Anchor Tavern in the Strand, according to annual custom.

LX. *Intelligence and Miscellaneous Articles.*

On the 25th of March as Mr. John Barnes, a Gravesend pilot, was proceeding in his boat to the Nore, he observed the sea unusually agitated a few miles below Gravesend, and approaching the spot, he perceived a whale struggling in the water: he immediately fired a swivel three different times; the second shot struck it in the tail, and the third wounded it mortally in the body, when, by a violent and sudden plunge, it threw itself on the beach, and was left nearly dry at low water. Four hours elapsed from the time it was wounded until it was perfectly dead, and it was towed next tide to Gravesend, where it was exhibited during four days. It was then brought up the Thames above London Bridge in a large barge, into which it had been previously put by the barge being scuttled and sunk, and afterwards floated to the surface, the whale having been first towed over it at high water, so that on the ebb of the tide it was left in the barge.

The scuttles were then plugged up, and the barge floated on the return of the tide.

This is the only animal of the kind seen in the Thames since the year 1780, when a whale 90 feet in length was killed near the same place.

Many seamen who have been in Greenland, after examining the whale in question, pronounced it to be a young one not exceeding a year old; yet its dimensions were as follow:

Extreme length from the lower jaw to the end of the tail 76 feet six inches; from the lower jaw to the end of the body at the tail 69 feet; lower jaw longer than the upper jaw 1 foot 4 inches; end of upper jaw to its eye 14 feet; from the upper jaw to its dorsal fin 48 feet 2 inches; length of dorsal fin at the base 4 feet; height of the dorsal fin 2 feet; from the body to the end of the tail 7 feet 6 inches; extremity of the tail 15 feet; circumference of the body at the dorsal fin 21 feet; eye placed from the spiracle 5 feet; length of the mouth from the jaw 16 feet 6 inches; length of pectoral fin 6 feet, breadth of ditto 2 feet; longitudinal lines (almost straight) beginning under the mouth to the middle of the fish; length of its eye 5 inches; colour of its laminæ whitish towards the back behind; distance of the eye to the mouth 5 inches; 6½ feet to the pectoral fin from back bone; outer skin peeled off, thickness of fine writing-paper; from one eye to the other 9 feet 9 inches; breadth of the lines on the belly 3 inches; orifice of its ear 3 inches; from its eye to its ear 3 feet 2 inches. It is pronounced by naturalists to be the *Balena boops*, or pike-headed species.

After being exhibited for about eight days to the inhabitants of London, it was put up to public sale and produced seventy-five pounds.

LECTURES.

Dr. CLUTTERBUCK will commence his Summer Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Monday the 5th of June, at Nine o'Clock in the Morning, at his House, No. 1, Crescent, New Bridge Street, Blackfriars; where further particulars may be had.

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METEOROLOGICAL TABLE,

By Mr. Carey, of the Strand,

For April 1809.

Days of the Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.			
March 27	37°	55°	43°	29·32	52	Cloudy
28	43	49	42	·50	0	Rain
29	42	45	40	·76	30	Cloudy
30	37	44	41	·79	34	Cloudy
31	43	45	39	·72	30	Cloudy
April 1	37	44	35	·76	51	Cloudy
2	36	44	32	·80	52	Shower of sleet
3	33	43	32	·86	54	Ditto ditto
4	31	40	30	30·05	30	Ditto ditto
5	30	42	32	·25	41	Ditto ditto
6	33	46	40	·14	38	Cloudy
7	40	46	39	·20	34	Cloudy
8	39	51	43	·30	48	Fair
9	43	53	44	·03	35	Cloudy
10	44	56	47	29·72	51	Cloudy
11	47	47	35	·38	25	Showery
12	36	49	42	·75	27	Fair
13	43	52	40	·21	37	Showery
14	42	49	40	·09	0	Thunder with hail
15	40	49	42	·56	41	Cloudy
16	48	54	41	·06	47	Showery
17	40	41	33	·22	0	Rain
18	33	43	32	·60	31	Storms of sleet
19	32	45	38	·72	51	Fair
20	33	44	33	·66	36	Storms of sleet a great fall of snow during the night.
21	33	47	45	·55	30	Cloudy
22	44	47	43	·70	28	Cloudy
23	43	46	42	30·05	21	Cloudy
24	40	47	39	·30	25	Cloudy
25	39	47	46	·25	26	Cloudy
26	46	49	46	29·82	0	Rain

N. B. The Barometer's height is taken at one o'clock.

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FRENCH INSTITUTE.

LXI. *Report on the ponderous Flint Glass intended for the Manufacture of Achromatic Glasses. Presented to the Institute by M.* DOUFOURGERAIS, *Optician to His Majesty the Emperor and King.—Laid before the Class of Mathematical and Physical Sciences, by Messrs.* DE PRONY, GUYTON, *and* ROCHON, *on Tuesday April* 10, 1809.

WE know that the invention of achromatic glasses is one of the grandest discoveries of the last century. We are indebted for it to a celebrated geometrician, who has enriched the mathematical sciences with the most astonishing conceptions. In 1747 Euler entertained the sublime idea of correcting, by the employment of several diaphanous substances, the aberration resulting from the decomposition of light in spherical glasses. This was the more valuable, because philosophers had been led to believe, according to the experiments of Newton, that there was no refraction when there was no dispersion,—thus banishing all hope of destroying the colours in glasses.

Euler informs us, in the Memoirs of the Academy of Petersburgh, that some experiments made upon meniscus glasses, the concavities of which he filled with various liquors, proved that the different refrangibility of the rays of light could be diminished, and even reduced to nothing, (these are the expressions of this great man, whose modesty was equal to his talents,) by employing two or more transparent substances. He adds, what is very remarkable, that the wonderful structure of the eyes, which represent the images of objects from their posterior extremity, suggested to him that it would be possible to diminish, and even to annihilate, all the defects to which the different refraction of the rays of light at that time seemed to be necessarily subjected. "Here again," says Euler, "we recognise the power of the Deity, as well as his infinite wisdom." He informs us at the same time, that his opinion was attacked by John Dollond, an eminent optician of London: but upon some observations of M. Klingenstierne, he ascertained, after a mul-

tiplicity

tiplicity of experiments, that the great inequality of the dispersive powers which takes place in two kinds of glass, vulgarly known by the names of *flint* and *crown* glass, was sufficient for realizing the idea of Euler, and thereby good achromatic glasses were obtained. Dollond's success procured him a patent in 1759, wich was called in question, however, by Valtines in Westminster Hall. Valtines proved that the ingenious Chester Morehall had constructed glasses long before Dollond, perfectly achromatic, and of an immense amplifying power. So early as the year 1754, M. Ayscough, an optician of Lancaster, possessed one of these instruments, as did also Dr. Smith. These facts, although but little known, deserve to be published, and were authenticated by lord Mansfield, who maintained Dollond in his privileges, merely because the benefit of a patent does not belong to him who has the first scientific idea of an invention, but to him who enables the public to enjoy the advantages of the discovery.

So far Mr. Dollond deserved a recompense; and the celebrated achromatic instrument with a triple object glass which he presented to the Royal Society excited a great sensation in the scientific world.

The Academy of Sciences, on being informed that instruments were made at London upon the principles of Euler, and which magnified one hundred times the diameter of the objects with the degree of clearness and distinctness requisite in delicate observations, proved by ingenious inquiries, that it attached the highest possible value to the new discovery in question. Two eminent geometricians, Messrs. Clairaut and D'Alembert, have left nothing to desire upon the intricate theory of the construction of these instruments. They fixed the spherical curvatures of glasses of unequal dispersive forces, which reduce to the minimum the aberrations of refrangibility and sphericity.

M. Clairaut afterwards ascertained by experiments, that the lapidaries of Paris, who endeavoured to imitate the diamond in their glassy compositions, made use of a kind of glass vulgarly known by the name of *strass*; the dispersive power of which is greater than that of flint glass. But this glass, to which an artist of the name of Strass had given, by means of the oxide of lead, a gravity equal to that of the diamond, is generally so soft (*gelatineux*), that it is very difficult to succeed in employing it usefully in the manufacture of achromatic object glasses, which require glasses not only perfectly homogeneous, but also blown glasses, according to the remarks of the most eminent opticians, who have discovered, in the practice of their art, the advantages of these blown glasses over those which were run into crucibles.

M. Loysel, in his Essay upon Glass Making, gives us the composition of a glass imitating the dispersion of the diamond: it is, he informs us, with 100 parts of white sand, washed in muriatic acid, combined and fused with 150 parts of red oxide of lead, to which must be added 30 parts of aërated and calcined potash, and ten parts of calcined borax, that the lapidaries produce, in small furnaces, the crystal which imitates the diamond, and which has the same weight; its specific gravity being as to water 35 to 10. They sometimes add one part of oxide of arsenic; but this composition, which they allow to cool in crucibles, produces but very small masses, which can only be used in making trinkets.

If, in the origin of the invention of achromatic glasses, M. Clairaut made use of this glass in the construction of some achromatic object glasses, it was because he was desirous to make an useful application of his formulæ upon glasses the dispersive power of which was much greater than that of flint glass: but M. de l'Etang, whom he charged with this work, remarked to him that blown glass, such as flint and crown glass, was requisite for good object glasses. On this account the Academy of Sciences, at the suggestion of the government, who were unwilling that France should be tributary to England in the article of glass, proposed in 1766, as the subject of a prize dissertation, the best process for imitating in France a ponderous glass exempt from all defects, and having all the properties of flint glass.

This prize was granted in 1773 to M. Lebaude, the manager of a glass-work, and his Memoir was printed among the Mémoires des Savans Etrangeres for 1774.

M. Lebaude, on this occasion, merely produed specimens of ponderous glass, and which could not satisfy the wants of opticians: the Academy was therefore obliged, in 1786, to renew the same subject as a prize dissertation, and the sum offered was 12000 livres. In announcing the subject proposed, a process was required, by means of which the quantity of ponderous glass necessary to supply the wants of commerce might be constantly supplied, and without the defects of flint glass.

Since the above period, essays have certainly been presented, but they were either too imperfect or too meagre for attaining the essential object which government had in view; namely, that of furnishing France with all the ponderous glass requisite for optical instruments, without having recourse to foreigners. This enterprise was not unattended with difficulties in its execution, because the heads of great glass-works, who alone could enter into these delicate and difficult inquiries with any chance of success, could not flatter themselves that the sale to opticians, of ponderous glass blown without any defects, was likely to reimburse them for the enormous expense this degree of perfection requires. This consideration is sufficient to show, that we cannot assimilate simple essays with works which should serve to vivify and extend an important branch of industry and commerce.

M. Doufourgerais, manufacturer of glass to the emperor, already known by the celebrity of his manufactory of crystal glass at Mont Cenis, has certainly excited a lively interest among us, on account of the decided preference which the produce of his industry has generally obtained over the glass of England and Bohemia, although he had to surmount many powerful obstacles. The Institute could not view without extreme satisfaction the works on a great scale which this ingenious and zealous manufacturer has recently executed. One specimen consists of 600 kilogrammes of a glass weightier than flint glass. It is 9000 millimetres thick, and 270 ~~centi~~metres high. He has already sold upwards of 300 kilogrammes of it to opticians, and the remainder will soon be bought up also, without the price being adequate to the capital generously sacrificed for the attainment of an object, the importance and utility of which he knew how to appreciate.

We shall now give an account to the Institute of the nature and quality of the ponderous glass which has been submitted to our examination.

We ought to premise, that the most eminent opticians are fully satisfied of the qualities of the glass in question, and that a great number of achromatic telescopes have been made with it. We now call the attention of the Institute to a letter to M. Doufourgerais from M. de Freminville, chief engineer of roads and bridges, who is specially charged with furnishing the telegraphs and the navy with the glasses required for the observation of signals.

" Pieces of glass from your magazine, taken at random, and subjected to the necessary operations for employing them as optical glasses, produced object glasses comparable to the best of Dollond's of the same dimensions. You have therefore attained, and I am proud to bear witness to it, the highest degree of perfection ever possessed by English glass, whether I consider it in a commercial or scientific point of view, since in your article beauty and utility are united to cheapness."

This impartial testimony from a person well acquainted with optical instruments, we are happy in being able to corroborate from our own experience. The glass made by M. Doufourgerais is heavier than flint glass: one of us measured the gravity of the former in the hydrostatic balance, and found it to be 3,588 with respect to distilled water, while the heaviest flint glass is only 3,329.

A prism of the glass made by M. Doufourgerais, having an angle of two degrees, ceases to colour objects the instant we place it against a prism of common glass, (such as the blown glass made at Cherbourg, which differs very little from crown glass,) when its angle is 18 degrees: from the experiments therefore of one of your committee, it appears that the dispersion which takes place in the glass made by M. Doufourgerais, is, to that observed in the most ponderous flintglass, as 36 to three. The mean refraction is also stronger, being 161, while that of flint glass is 160.

We caused a piece of the new glass to be cut into lenses of 160 millimetres focus; and we can assure the Institute, that this rigorous examination convinced us that France may now dispense entirely with flint glass in the construction of good achromatic glasses, so necessary in the naval and military service. The above glasses, which we have examined and compared with English glasses, prove that the eulogies we bestow on them are well founded: we do not mean to say, however, that, when taken indiscriminately, the glass made by M. Doufourgerais can be used for the large object-glasses used by astronomers in delicate observations: in this case, as in flint glass, it would be necessary to pick out a piece, in order to avoid the air-drawn threads and streaks from which blown glass is rarely free; and we ought not to require, in a large mass of glass, a perfection which is perhaps chimerical, for instruments of no importance in a commercial point of view, however valuable in scientific pursuits. We must observe, nevertheless, that the glass made by M. Doufourgerais, although very ponderous, has generally fewer threads than flint glass, and its clearness equals, if it does not surpass, that of English glass.

The largest glass made with the article manufactured by M. Doufourgerais, and which we examined, was only eight decimetres in length. Its object glass had an aperture of 60 millimetres, and it magnified the diameter of objects 30 times. With astronomical eye-glasses we can make it exhibit a much greater magnifying power: but this increase is not of any use in the examination of terrestrial objects. We doubt if it has all the aperture which it can support, because opticians must change the ordinary proportions, when they use a glass which has a stronger dispersion than *strass*.

In 1774, Nicholas Fust published a work in French, with the following title: "*Instruction détaillée pour porter les Lunettes de toutes les différentes Espèces au plus haut Dégré de Perfection dont elles sont susceptibles, tirée de la Théorie dioptrique*

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Phil. Mag. Vol. XXXIII. Pl. VIII.

Major (Le Hardy's Telegraph)

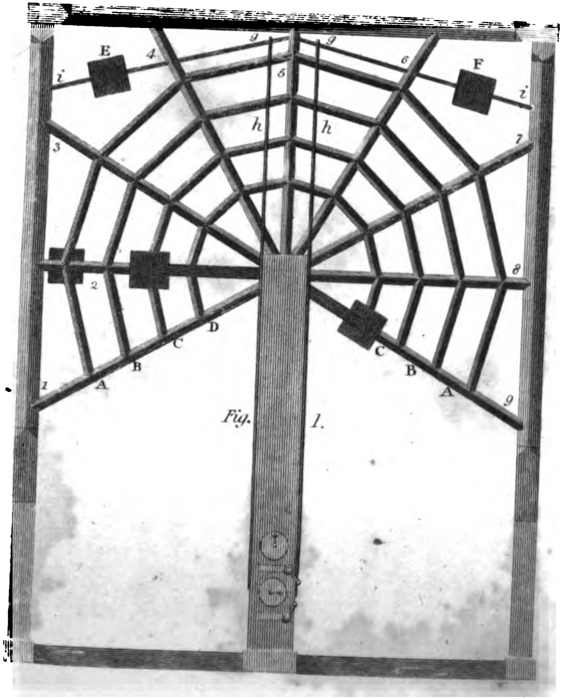

Fig. 1.

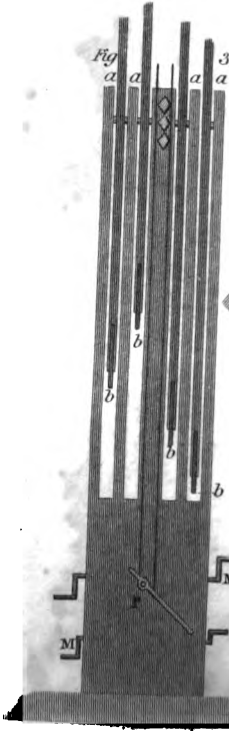

Fig. 3.

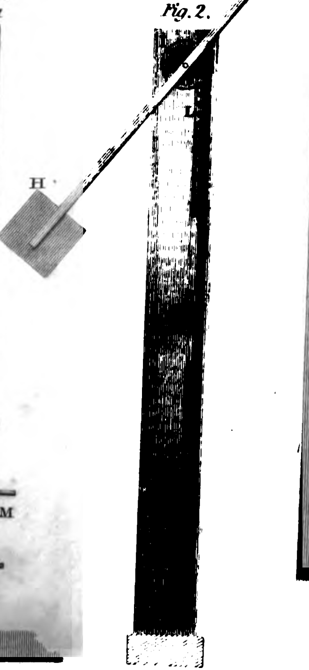

Fig. 2.

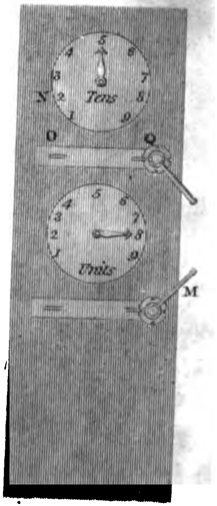

Fig. 4.

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dioptrique de M. Euler, *et mise à la Portée de tous les Ouvriers en ce Genre.*" This work ought to be in the hands of every optician: but it is scarce in France, having been printed at Petersburg.

Artists will learn, from the above book, the advantages and changes which it is important to observe in the use of a kind of glass with a greater dispersive power than that of flint glass. But these inquiries are interesting to opticians only, and are foreign to the labours of M. Doufourgerais, who seems to us to merit in every respect the encouragement and protection of the Government, as well as the approbation of the Institute.—Signed De Prony, Guyton, Rochon.

The Class approves of the above report, and assents to its conclusions.

Signed, Delambre, perpetual secretary.

LXII. *Description of an improved Telegraph. By Major* Charles Le Hardy, *of the Island of Jersey**.

Sir,

Having discovered a mode of communicating by signals, which to me seems to unite every advantage that can be expected for that mode of communication, I beg leave to transmit to you a plan of it, and to write a few explanatory lines upon the subject.

The telegraph, of modern invention, is an improvement on communication by signals, which has been in use for many ages. Monsieur Chappe was, I believe, the first who proposed a machine for that purpose, which was put in execution about the year 1793. Since that time several modes have been proposed by different persons, none of which seem to have fully attained the object. A machine of this kind should be simple, and easy to comprehend. All those which have come to my knowledge are executed by combinations, which render them too complicated for common use,

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* From *Transactions of the Society for the Encouragement of Arts, Manufacture, and Commerce*, for 1808.——The silver medal of the Society was voted to Major Charles Le Hardy for this communication.

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use, and therefore liable to many errors. That which I now submit to your consideration, seems to me to have removed every objection of the kind; it is simple, easy to comprehend, and extensive in its means; its combinations, which by simple numeration may be carried to 40,000, might with ease be extended to almost infinite numbers; but the present seems sufficient to answer every purpose. All the words in Entick's Dictionary amount to about 25,000; every one of which may thus be numbered. With how much more dispatch would a letter be communicated by signals which express words, than by signals which express only letters! Words may be forwarded as fast as they can be looked for in a dictionary; and even whilst only an equal number of letters could have been communicated by the present mode. Another advantage resulting from the use of words in telegraphic correspondence is, that the words of the same meaning in the several languages having the same number, correspondence may be carried on from one language into another, which though not grammatically correct, yet would be sufficiently intelligible.

Proper names must be spelt, which may easily be done, every letter having a corresponding number.

Though the use of telegraphs has to this time been confined to military purposes, yet a machine of this kind is well adapted to accelerate commercial communication from one end of the kingdom to the other. The arrival, the departure of vessels, the various transactions of commerce, might be speedily announced, to the very great advantage of trade. By this method inaccessible places might communicate their wants, and correspondents, though at a distance of five or six miles, might erect them for a trifling expense. I made the experiment with one of eight feet by ten, and, with the use of a telescope, I took down every number from a distance of a mile and a half.

I remain, sir,

Your most obedient and humble servant,

CHARLES LE HARDY.

London, Jan. 13, 1808.

To C. TAYLOR, M.D. Sec.

Reference

Reference to the Engraving of Major Charles Le Hardy's *Telegraph.* *See* Plate VIII. Figs. 1, 2, 3, and 4.

This machine is intended to express numbers, which may be seen at a distance, and to which words may be referred at pleasure.

Fig. 1 represents a front view of the machine—it is composed of nine bars or radii, answering to the nine figures of arithmetic, as numbered in the plate. The four polygonal or concentric bars, A. B. C. D. which intersect the radii, are for the decimals; thus A stands for units, B for tens, C for hundreds, D for thousands. Over each of these concentric bars or circles, an index, as that marked H, fig. 2, traverses, which marks the number of thousands, of hundreds, of tens, and of units, as far as ten thousand; for instance, if it is required to make the number 9202, turn the hand H in the circle D of thousands to the radius 9; then the hand H in the circle C of hundreds to the radius 2, then, as there are no tens, turn the hand to the radius 2, upon the circle A of units; but as ten thousands are not sufficient to express the number of words in the English language, two square boards are added in the corners, of which that marked E is equal to 10,000, that marked F to 20,000, and both being shown together, are equal to 30,000, which, with the numbers made on the circles, bring it up to 40,000, which number is more by many thousands than all the words in the English language.

Fig. 2 is a view of the mechanism which works the signals round: this is done by means of a rack wheel at I, upon which is firmly fixed the hand with its signal board H; this wheel is made to revolve by means of a rack L, which being raised or lowered, makes it go backwards or forwards; this rack is set in motion by the pinion K, to which is fixed a winch, as M, fig. 3. To prevent the necessity of inspecting the signals, the wheel I, to which is fixed an index, as at N, fig. 4, is added, which revolving in the same time as the signal board H, marks the number of the decimal, so that it may be worked correctly from within doors. O, fig. 4, is a bolt to stop the hands at any given point, by means of a wheel with four notches Q fixed to the pinion. Fig. 3 is a side section of the machine, the lines *a a a a* represent boards, upon each of which is fixed the mechanism, fig. 2. *b b b b* are the hands and signal boards, one of which is shown at H, fig. 2. The square boards E and F, fig. 1, are fixed upon iron bars which pass through the bars or radii 4 and 6, and have each of their ends made to move in holes in the radius 5, and in the frame at *i i*, fig. 1, upon which they turn a quarter round, by means of cross bars at *g g*, to each end of which ropes or wires *k k* are fastened, and which are connected with two levers, one of which is shown at P, fig. 3, the raising or depressing of which makes them appear or disappear as required.

LXIII. *Description of an Improvement in Jury Masts.* By Capt. William Bolton, *of the Royal Navy**.

SIR,

Herewith you will receive the model of a plan for fitting ships' jury masts, to be formed from the spare spars usually carried on board king's ships, and in every merchantman that is properly found. By having jury masts so fitted, ships will be enabled to carry as much sail as on the usual regular mast; the great use of which I need not dwell on, only observing that it may be of great importance to fleets after a general action, or when in want of proper lower masts, either at home or abroad, and enable ships, after the loss of their mast, to prosecute their voyage, or service, without any deficiency of sail.

I beg you will be pleased to lay it before the Society, and I have the honour to be,

Sir, your obedient humble servant,

Wm, Bolton.

His Majesty's Ship Fisgard,
Sheerness, Oct. 31, 1807.

To C. Taylor, M.D. Sec.

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* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1808.——The silver medal of the Society was voted to Capt. William Bolton for this communication.

REMARKS.

Fig. 4.

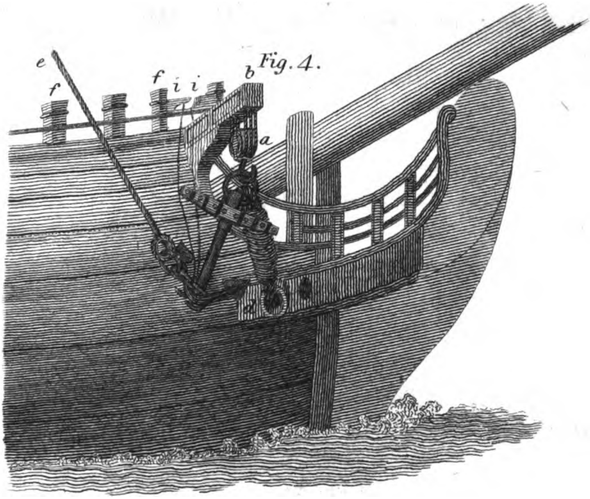
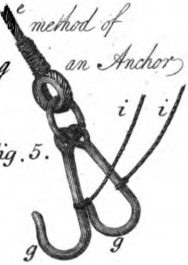
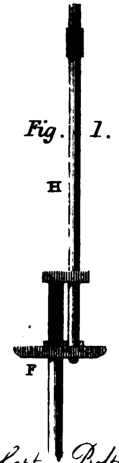


Fig. 5.



Capt. Ball's method of Fishing an Anchor.

Fig. 1.



Capt. Bolton's improved Jury mast.

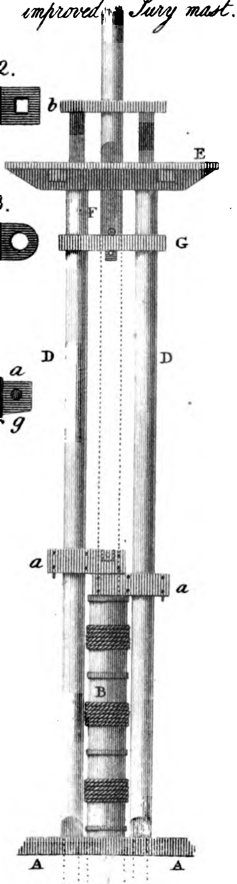


Fig. 2.

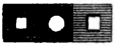


Fig. 3.

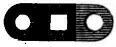


Fig. 6.

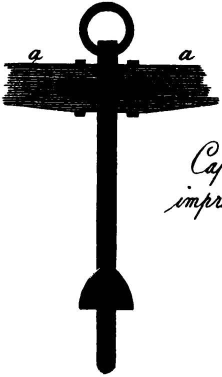
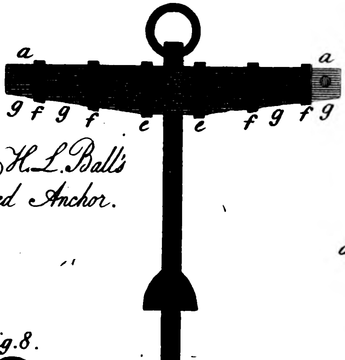
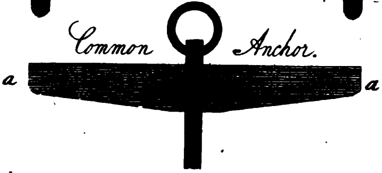


Fig. 7.



Capt. H. L. Ball's improved Anchor.

Fig. 8.



Common Anchor.

S. Porter sculp.

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REMARKS.

In the model in the Society's possession the main mast is broken about one-third of its length above the deck, proper partners are secured on the deck, in which a hand mast and spare main top mast are fixed on each side of the broken main mast, and secured thereto by two spare caps, morticed on a square made in its centre. A strengthening cap, moveable on these additional masts, connects them, and the upper parts of these masts are secured firmly by trustle trees in the main top. The foot of a spare fore top mast passes through a cap made from strong plank, morticed into the heads of the two temporary masts above mentioned, goes through the main top, and rests in the moveable strengthening cap, which connects those two masts, and enables the fore top mast to be raised to any height which the main top will admit, and be then firmly secured by the upper cap, the main top, and the strengthening cap below it. The fore top mast being thus adjusted, the cross trees and top gallant mast are mounted upon it, which completes the whole business.

Two caps are the only things necessary to be made expressly for the purpose, the other articles being usually ready on board the ship.

Reference to the Drawing of Capt. Bolton's *Jury Mast.*

Plate X. Figs. 1, 2, 3.

Figs. 1, 2, and 3, where A A represents the partners or pieces of timber which are bolted to the quarter deck for the mast to rest upon. B is the stump of the lower mast, which is cut square at the top, and of the same size as the head of the mast originally was; upon this square, the main and spare lower caps *a a* are fixed; two mortices must be cut in the partners A A to receive squares made at the lower ends of the two temporary masts D D, which are supported by the caps *a a*; one of them is a spare main top mast, the other a hand mast; these two support the main top E, additional squares being made on the trustle trees to receive each of them. *b* is a cap shown in fig. 2, made of four-inch plank doubled for the purpose, and fitted upon the heads of the masts D D, for a top mast F F, the heel of which rests in a mortice made in the stump of the lower mast; it is also steadied by a double cap G, separately shown in fig. 3, on which it fids finally on the top. The top-gallant mast H is fixed to the mast F by the top and cap in the usual manner. The figures 2 and 3 show the caps separated from the masts, and are the only things necessary to be made for the purpose; and the object of the cap, fig. 2, is to steady and prevent any wringing of the lower jury mast, and to fid the top mast whenever it is reefed. The fore top mast F F appears in two separate pieces, on account of its length.

LXIV. *Improvement in Anchors, to render them more durable and safe for Ships; and an improved Mode of Fishing Anchors. By Capt.* H. L. BALL, *of the Royal Navy**.

SIR,

THE great expense of timber in the navy for anchor stocks, and the frequency of their failing or giving way in the centre, where the square of the anchor is let into the stock, have induced me to offer to the Society of Arts, &c., a plan of an anchor which may be cheaper in construction, and more likely to hold in various situations than those in common use.

The model I have sent will sufficiently explain my intention, and show how beneficial it may be in strengthening the anchor stocks. I wish much to notice to you its probability of holding in the ground longer than other anchors, on account of the additional weight of the stock; and this will more particularly be the case in banks which shelve suddenly down from the shore, such as at St. Helena, Cawsand Bay, and indeed in most of the islands in the West Indies. The proportion of additional iron, as explained by my model, is in all anchors to be twice and a half the diameter of the shank from each side at the stock, and of course this mode will supply the place of the present nuts, which are only intended to prevent the stock from slipping in and out, whenever it becomes loose; which accident anchors are very liable to in hot climates. My anchor stocks will save a considerable quantity of the finest timber, and give much greater security.

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* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1808.——The silver medal of the Society was voted Capt. H. L. Ball for this communication.

I likewise beg leave to offer to the Society a model of a double fish hook, for the purpose of fishing the anchor, an operation which, in the common mode of doing it, is frequently attended with accidents both to the ship and crew, from the anchor suddenly slipping unexpectedly in raising it to its proper position.

I flatter myself that these improvements will meet with the Society's approbation.

I am, sir, your most obedient humble servant,

H. L. Ball.

Lower Mitcham, Feb. 13, 1808.

To C. Taylor, M.D. Sec.

Reference to the Engravings of Capt. H. L. Ball's *Method of Fishing an Anchor. See* Plate X. Figs. 4 and 5.

Fig. 4, Pl. X., represents captain Ball's method of fishing an anchor. Fig. 5 shows an enlarged view of his double hooks used for that purpose.

In the usual operation of heaving an anchor, it is drawn up by the cable until it appears above water; the cable will not now raise it higher, it is therefore bowsed up by the cat block *a*, fig. 4, from the cat head *b*, the cable *d* being slackened out as it rises. When it is got up as high as the cat block will raise it, a strong hook, called the fish hook, fastened to a rope *e*, which is suspended by a tackle from the shrouds, is hooked to the anchor at the bottom of the shank, and thus the arms of the anchor are elevated above the stock, until one of the flukes is brought up to the timber heads *f f*, to which it is made fast by a rope and chain, called the shank painter. In this operation the fish hook sometimes slips, and occasions mischief; to remedy which, captain Ball has applied two hooks instead of one, which keep firmer hold. These hooks are shown upon an enlarged scale at *g g*, fig. 5, attached to the rope *e*; each of these hooks takes one of the arms of the anchor, close to the shank, and holds it firmly. *i i* are two small lines made fast to the hooks, to direct them so as to get proper hold of the anchor.

Reference to the Engraving of Captain H. L. BALL's *Improvement in the Formation of Anchors. See* Plate X. Figs. 6, 7, and 8.

This anchor, in external appearance, differs very little from the common anchor; the improvement consists in forming and fixing of the shank of the anchor to the stock. The stock *a a*, figs. 6 and 7, is made of two pieces of oak bolted together, and well secured by hoops. In the common method, in order to prevent the anchor stock from slipping off the shank, a square projection *b b*, fig. 8, is forged upon the shank; this is improved by captain Ball, as shown in fig. 6, where this projection *d d* is extended on each side of the shank, far enough to receive two bolts through each of these extensions, which bolts hold firmly together the two pieces of timber which form the stock, and secure the stock fast to the shank. Two iron hoops, fig. 7, *e e*, are driven on the stock between the bolts, and *f f f f* are other hoops, and *g g g g* are tree-nails to strengthen the whole.

LXV. *Method of ascertaining the Value of Growing Timber Trees, at different and distant Periods of Time. By Mr.* CHARLES WAISTELL, *of High Holborn.*

[Concluded from p. 332.]

Observations on Tables I. *and* II.

THE preceding tables furnish us with the following useful information, viz.

1st. That all regular growing trees, measured as above, as often as their age is increased one-fourth, contain very nearly double their quantity of timber.

2d. That when a tree has doubled its age, its contents will be eight-fold.

3d. That when a tree has doubled its age, the annual growth will be increased four-fold.

4th. Consequently, that when a tree has doubled its age, the proportion that its annual increase bears to the contents of the whole tree is then diminished one-half.

This last observation explains how it comes to pass that a tree, when its age is doubled, the rate per cent. per annum that its increase then bears to the content of the whole tree, is diminished one-half.

It may not be unuseful to observe, that the rate per cent. of increase in the last columns, is the same as the rate per cent. that the increase of the tree that year will pay for the money it was worth the preceding year.

In the two preceding tables, we find that the rate of increase per cent. per annum is the same in both at the same ages, although the quantity of timber in the second table is six times as much as in the first table in trees of all ages: therefore, when the age of a tree is known, the rate per cent. per annum of its increase is known on inspecting these tables, whether the tree has grown fast or slow; provided the growth of the tree has been regular, and that it has continued its usual growth.

And having the age, girt, and height, of any tree given, we can readily calculate what quantity of timber it will contain at any future period whilst it continues its usual rate of growth.

A Table showing the Number of Trees to be cut out in thinning of Woods, and the Number left standing at every Period of 4 Years from 20 up to 64 Years.

In the 24th Volume of the Transactions of the Society of Arts, &c. page 75, Mr. Salmon, in a paper on the Management of Fir Woods, says, "the distance of trees from each other should be one-fifth of their height." At that distance, which is probably sufficient for fir trees, the following will be the number on an acre, and the number to be cut out at the ages and heights under mentioned, and the number of feet they will then contain in the bole, when measured to the top of the leading shoot. These trees are supposed to increase twelve inches in height, and one in circumference, annually, and to have been at first planted four feet apart.

TABLE III.

Years old and feet high.	Girt.	Contents.			Distance.	Number of Trees on an Acre.	Contents of the whole.	Number to be cut out.	Contents.
	inch.	ft.	in.	pts.	feet.		feet.		feet.
20	2½	0	10	5	4·	2722	2362	839	727
24	3	1	6	0	4·8	1883	2824	494	741
28	3½	2	4	7	5·6	1389	3308	326	776
32	4	3	6	8	6·4	1063	3779	223	792
36	4½	5	0	9	7·2	840	4252	160	810
40	5	6	11	4	8·	680	4722	118	819
44	5½	9	2	11	8·8	562	5194	90	831
48	6	12	0	0	9·6	472	5664	70	840
52	6½	15	3	0	10·4	402	6130	55	838
56	7	19	0	8	11·2	347	6611	45	857
60	7½	23	5	2	12·	302	7076	37	866
64	8	28	5	4	12·8	265	7537		

And if trees be periodically thinned out to the distance of one-fifth of their height, and that they increase fifteen inches in height, and one inch and a half in circumference, annually, the number of trees on an acre, and the number to be cut out at different periods, and the number of feet they will respectively contain at those periods, will be as under, viz.

TABLE IV.

Age.	Hght.	Girt.	Contents.			Distance.	Number of Trees on an Acre.	Contents of the whole.	Number to be cut out.	Contents.
years.	feet.	inch.	ft.	in.	pts	feet.		feet.		feet.
16	20	3	1	3	0	4	2722	3402	980	1225
20	25	3¾	2	5	3	5	1742	4246	532	1296
24	30	4½	4	2	7	6	1210	5100	322	1357
28	35	5¼	6	8	4	7	888	5944	208	1392
32	40	6	10	0	0	8	680	6800	143	1430
36	45	6¾	14	2	10	9	537	7644	102	1452
40	50	7½	19	6	4	10	435	8494	75	1464
44	55	8¼	25	11	10	11	360	9355	58	1507
48	60	9	33	9	0	12	302	10192	45	1518
52	65	9¾	42	10	10	13	257	11026	35	1501
56	70	10½	53	7	0	14	222	11895	29	1553
60	75	11¼	65	10	11	15	193	12720	23	1515
64	80	12	80	0	0	16	170	13600		

It will be observed in all these tables, that when trees have doubled their age, there are only one-fourth of the number remaining on an acre, in consequence of their distance being doubled; but as each tree will then have increased its contents eight-fold, therefore the number of feet on an acre must be then doubled. Above, at 64 years of age, there is exactly double the number of feet that there is at 32 years of age.

And if trees be periodically thinned out to the distance of one-fifth of their height, and that they increase eighteen inches in height, and two inches in circumference, annually, the number of trees on an acre, and the number to be cut out at different periods, and the number of feet they will then respectively contain, will be as under, viz.

TABLE V.

Age.	Hght.	Girt.	Contents.			Distance.	Number of Trees on an Acre.	Contents of the whole.	Number to be cut out.	Contents.
years.	feet.	inch.	ft.	in.	pt.	feet.		feet.		feet.
12	18	3	1	1	6	4·	2722	3062	839	943
16	24	4	2	8	0	4·8	1883	5021	673	1794
20	30	5	5	2	6	6·	1210	6302	370	1927
24	36	6	9	0	0	7·2	840	7560	223	2007
28	42	7	14	3	6	8·4	617	8817	145	2072
32	48	8	21	4	0	9·6	472	10069	99	2112
36	54	9	30	4	6	10·8	373	11314	71	2153
40	60	10	41	8	0	12·	302	12583	52	2166
44	66	11	55	5	6	13·2	250	13864	40	2218
48	72	12	72	0	0	14·4	210	15120	32	2304
52	78	13	91	6	6	15·6	178	16294	24	2197
56	84	14	114	4	0	16·8	154	17607	20	2286
60	90	15	140	7	6	18·	134	18843	16	2250
64	96	16	170	8	0	19·2	118	20138		

But if the trees be first planted four feet apart, and be periodically thinned out to the distance of one-fourth of their height, and that they increase twelve inches in height, and one in circumference, annually, the number of trees on an acre, and the number to be cut out at the ages and heights under mentioned, and the number of feet they will respectively contain in the bole, when measured to the top of the leading shoot, will be as under, viz.

TABLE VI.

Years old and feet high.	Girt.	Contents.			Distance.	Number of Trees on an Acre.	Contents of the whole.	Number to be cut out.	Contents.
	inch.	ft.	in.	pt.	feet.		feet.		feet.
16	2	0	5	4	4	2722	1209	980	435
20	2½	0	10	5	5	1742	1512	532	461
24	3	1	6	0	6	1210	1815	322	483
28	3½	2	4	7	7	888	2115	208	495
32	4	3	6	8	8	680	2417	143	508
36	4½	5	0	9	9	537	2718	102	516
40	5	6	11	4	10	435	3020	75	520
44	5½	9	2	11	11	360	3327	58	536
48	6	12	0	0	12	302	3624	45	540
52	6½	15	3	0	13	257	3919	35	533
56	7	19	0	8	14	222	4230	29	551
60	7½	23	5	2	15	193	4522	23	538
64	8	28	5	4	16	170	4835	20	568
68	8½	34	1	4	17	150	5116	16	545
72	9	40	6	0	18	134	5427	14	567
76	9½	47	7	6	19	120	5715	12	571
80	10	55	6	8	20	108	6000	10	555
84	10½	64	3	8	21	98	6301	8	554
88	11	73	11	4	22	90	6655	8	591
92	11½	84	5	11	23	82	6928	7	591
96	12	96	0	0	24	75	7200	6	576
100	12½	108	6	0	25	69	7486	5	542
104	13	122	0	8	26	64	7811	5	610
108	13½	136	8	3	27	59	8037	4	546
112	14	152	5	4	28	55	8384	4	609
116	14½	169	4	5	29	51	8659	3	508
120	15	187	6	0	30	48	9000	3	562
124	15½	206	10	7	31	45	9309	3	620
128	16	227	6	8	32	42	9557	2	455
132	16½	249	6	8	33	40	9982		

And if the trees be periodically thinned out to the distance of one-fourth of their height, and that they increase 15 inches in height, and one inch and a half in circumference annually, the number of trees on an acre, and the number to be cut out at the different periods under mentioned, and the number of feet they will respectively contain at those periods, will be as under, viz.

TABLE

TABLE VII.

Age.	Hght.	Girt.	Contents.			Distance.	Number of Trees on an Acre.	Contents of the whole.	Number to be cut out.	Contents.
years.	feet.	inch.	ft.	in.	pt.	feet.		feet.		feet.
12	15	2¼	0	6	3	4·	2722	1417	980	510
16	20	3	1	3	0	5·	1742	2177	627	788
20	25	3¾	2	5	3	6·25	1115	2717	341	831
24	30	4½	4	2	7	7·5	774	3262	206	868
28	35	5¼	6	8	4	8·75	568	3802	133	890
32	40	6	10	0	0	10·	435	4350	91	910
36	45	6¾	14	2	10	11·25	344	4897	66	938
40	50	7½	19	6	4	12·5	278	5428	48	937
44	55	8¼	25	11	10	13·75	230	5976	37	962
48	60	9	33	9	0	15·	193	6513	29	978
52	65	9¾	42	10	10	16·25	164	7036	22	948
56	70	10½	53	7	0	17·5	142	7608	19	1018
60	75	11¼	65	10	11	18·75	123	8106	15	988
64	80	12	80	0	0	20·	108	8640		

And if the trees be planted at 4½ feet apart, and be periodically thinned out to the distance of one-fourth of their height; and that they increase 18 inches in height and two inches in circumference annually, the number of trees on an acre, and the number to be cut out at the different periods under mentioned, and the number of feet they will then respectively contain, will be as under, viz.

TABLE VIII.

Age.	Hght.	Girt.	Contents.			Distance.	Number of Trees on an Acre.	Contents of the whole.	Number to be cut out.	Contents.
years	feet.	inch.	ft.	in.	pts.	feet.		feet.		feet.
12	18	3	1	1	6	4·5	2151	2419	941	1058
16	24	4	2	8	0	6·	1210	3226	436	1162
20	30	5	5	2	6	7·5	774	4031	237	1234
24	36	6	9	0	0	9·	537	4833	142	1278
28	42	7	14	3	6	10·5	395	5645	93	1329
32	48	8	21	4	0	12·	302	6442	63	1344
36	54	9	30	4	0	13·5	239	7249	46	1395
40	60	10	41	8	0	15·	193	8041	33	1375
44	66	11	55	5	6	16·5	160	8873	26	1441
48	72	12	72	0	0	18·	134	9648	20	1441
52	78	13	91	6	6	19·5	114	10435	16	1464
56	84	14	114	4	0	21·	98	11204	12	1372
60	90	15	140	7	6	22·5	86	12093	11	1546
64	96	16	170	8	0	24·	75	12800		

It is difficult in thinning plantations to leave the trees at nearly equal distances. The distances stated in all these tables must be considered the average distances. If, for instance, there be 302 trees on an acre, their average distance will be 12 feet, although few of them may stand at exactly that distance.

If the trees be first planted four feet apart, and be periodically thinned out to the distance of one-fourth of their height until they are 28 feet high, and to one-third of their height afterwards, and that they increase 12 inches in height and one in circumference annually, the number of trees on an acre, and the number to be cut out at the ages and heights under mentioned, and the number of feet they will then respectively contain in the bole, when measured to the top of the leading shoot, will be as under, viz.

TABLE IX.

Years old and feet high	Girt.	Contents.			Distance.	Number of Trees on an Acre.	Contents of the whole.	Number to be cut out.	Contents.
	inch.	feet	in.	pts.	feet.		feet.		feet.
16	2	0	5	4	4	2722	1209	980	435
20	2½	0	10	6	5	1742	1512	532	461
24	3	1	6	0	6	1210	1815	322	483
28	3½	2	4	7	7	888	2115	453	1078
30	3¾	2	11	1	10	435	1271	133	388
36	4½	5	0	9	12	302	1528	80	404
42	5¼	8	0	5	14	222	1783	52	417
48	6	12	0	0	16	170	2040	36	432
54	6¾	17	1	0	18	134	2289	26	444
60	7½	23	5	2	20	108	2530	18	421
66	8¼	31	2	4	22	90	2807		

Observations on Table IX.

On examining several oak woods, it appeared to me, that the distance of one-third of their height was not too much where the trees were from 30 to 40 feet high and upwards. I have therefore calculated a table according to the distance of one-fourth of their height, till they are 28 feet high, and according to the distance of one-third of their height afterwards.

The timber to be thinned out before the age of 28 years, will be the same as in Table VI., but at 28 years of age there are 583 feet more to be cut out according to this Table than at the same age in Table VI.; there will, however, be less to be cut out between the ages of 28 and 60 years of age. But if the trees in this Table, in consequence of having more room, were to increase 1¼ inch in circumference annually, instead of one inch after they are 28 years of age, the produce of an acre at 60 years of age would equal the produce stated in Table VI. at the same age; taking into consideration that the value of the 583 feet excess cut out at 28 years of age would then be more than quadrupled, if the money were placed out at five per cent. compound interest. A considerable additional increase in circumference may certainly be expected, in consequence of the trees having almost double the room in which to extend their branches, and for the admission of those powerful agents, sunshine and air.

Observations on the Tables respecting the Thinning of Woods, and their Produce.

Mr. Salmon is the only person I know of, who has given a general rule for thinning plantations. But as I conceive his distance of one-fifth of their height would leave oaks too close, especially after they had acquired a sufficient length of stem, I have calculated both on his plan, which is proper for fir trees, and also at greater distances.

The preceding Tables VI. VII. and VIII. are calculated on a supposition that the trees are never suffered to stand nearer, on an average, than one-fourth of their height; and although the quantities of timber thinned out and left standing on the ground at that distance, at the end of 60 years, be only two-thirds of the quantity according to Mr. Salmon's distance, yet I suppose it will be generally thought an ample produce and sufficiently encouraging.

According to Table VI. which is calculated for oaks, the first thinning is at sixteen years old, and the second at twenty, but it is the advice of an eminent planter, (Mr.

Pontey,)

Pontey,) to begin thinning at about thirteen years old, according to the state of the trees, and to cut out about 150 poles per acre annually, for the next seven years. Without putting any value upon the thinnings before 20 years old, we find that at the 20th and 24th years the thinnings measure 945 feet, the value of which, at a low estimate, will be sufficient to repay the rent and taxes of ground of a moderate quality, the expense of plants, planting, and after-management, calculated at five per cent, compound interest.

When 28 years old, and at the end of every fourth year following, up to 120, the trees to be cut out of an acre will measure from 495 to 550 feet; but say 500, at 4*s*. a foot, on an average, including the value of the bark; this gives 100*l*. which sum divided by 4, leaves 25*l*. for the produce per acre per annum. This deserves the consideration of those who are inclined to convert young woods into coppices, without leaving a reasonable number of standards.

It may however be said, that as the trees cut out in thinning plantations are the bad thrivers and underlings, their contents will be less than the average; but, if we take their value at one-half the above estimate, that is, after the rate of 12*l*. 10*s*. per acre per annum at 28 years of age and upwards, even this produce must be thought ample, together with the value of the trees left standing.

Table VI. was constructed chiefly with a view to oaks, their annual increase in circumference varying from ¾ of an inch to 1¼ inch, the medium of which is one inch.

Tables VII. and VIII. were calculated for ash, elm, sycamore, firs, poplars, and other woods of swift growth, their increase in circumference being generally from 1½ to 2 inches annually. If ash trees be found to increase after the rates of Table VII. or VIII. they must be exceedingly profitable, at the high prices now given for that timber. Many other observations might be made on Tables VII. and VIII., but these will readily occur to persons interested in quick-growing trees.

An acre of trees increasing after the rate of Table VI. produces in 64 years little more than half the number of feet that another acre produces which increases after the rate of Table VII., and little more than one-third of another, increasing after the rate of Table VIII. in the same time.

In planting with a view to profit, the first object is a long, straight, and clear stem. This is most certainly and speedily obtained by thick planting at first, and not thinning too soon. A kind of competition among the trees is thereby occasioned, each struggling, as it were, to outgrow its neighbour, in search of light, heat, air, and moisture.

This competition must, however, be judiciously moderated by timely thinning; always keeping the trees sufficiently strong in the stem. If they be suffered to stand some years too near each other, their stems will become weak, and bend under their small tops when thinned. Where this has taken place in only a small degree, they will make but little progress for some years afterwards.

By the time the trees have advanced to 24 or 30 feet high this competition should cease, if they are intended to be cut down at or before 60 years of age, and they should then be encouraged to extend their tops more in width than in height, strong side branches being apparently quite as conducive as the leading shoot, to the vigorous growth of the bole below them. After this period, the best rule for thinning will probably be, to leave a clear space around the top of each tree, in which the branches may extend themselves without obstruction. A tree whose top is 20 feet diameter, receives four times the benefit from air, rain, and dew, as another does whose top is only ten feet diameter.

The trees in the interior of young woods are smaller in their boles than the exterior trees. And in a fine oak wood, of about 40 acres, divided into squares by several avenues or ridings crossing each other at right angles, I observed the rows of trees next the avenues much thicker in their boles than the trees in the interior of the squares; owing, no doubt, to their having more and larger branches in consequence of their having more room, although it is only on one side.

Being too parsimonious of ground seems to me a great and very general error. If the same number of trees of 30 feet

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feet high and upwards, in Table VI. were allowed the space of two acres instead of one, and, in consequence of their standing thinner, were to increase annually only the fiftieth part of an inch more in girt than they would do if they stood on one acre, this small additional increase in girt would pay an ample rent for the additional acre.

In the year 1791 a paper of Observations on the Propagation and Management of Oak Trees in general, but more particularly applying to his majesty's New Forest in Hampshire, was published by T. Nichols, Purveyor of the Navy for Portsmouth Dock-yard.

In this paper it is said, that "there are to be seen in many parts of the forest from 40 to 50 fine oaks standing on an acre, that will measure one with another two loads a tree."

"Several woods in the forest are almost ruined for want of thinning, and its being done at proper times; particularly the inclosures that were made in the year 1700:—these were originally well planted, and great numbers of trees brought up in them, which now remain so close together that they are nearly stagnated, particularly in Salisbury, Trench, Brimley Coppice, and Woodfidley; and although it is 90 years since they were planted, the trees will not measure, one with another, above six or seven feet a tree; whereas, if the business of thinning had been done as it ought, the remaining trees (after drawing much useful timber) would by this time have been of a size nearly fit for naval uses, as in some of the woods that were planted at the same time, the trees which have had room to expand, and a free air admitted to them, will measure from 70 to 80 feet."

Observations on the Growth of Timber.

The rings observable in the transverse section of a tree at its butt-end, are the same in number as the years of its age; an additional ring being produced annually, in consequence of the annual rising of the sap. The rings are nearly concentric in trees that have grown in the interior of close shady woods, but eccentric in others, being of different breadths on the northern and southern sides of such as have grown single, or in any other situation where their boles have been much exposed to the rays of the sun. This difference is occasioned by the different degrees of heat to which the opposite sides of the boles of trees are exposed. And, indeed, we find these rings are always broadest on that side of the bole or stem most warmed by the sun. Hence we see the utility of exposing their boles as much as possible to its rays*. It is often seen in the stumps of trees that have stood single, that they have grown nearly twice as fast on the southern side as on the northern, their pith being so much nearer to the northern side.

It is, however, to be remarked, that the wood from that side of a tree which has grown the slowest, is heavier than from the opposite side which has grown the fastest, and it is probably stronger in the same degree.

It may be worth the consideration of those who have southern hangs or declivities to plant, whether to plant, or rather leave the trees in thinning, in double rows in lines running east and west, at about fourteen or sixteen feet distance, and the double rows at about thirty-six feet distance, less or more, according as the declivity is more or less, in order that their boles may receive the greatest possible benefit from the direct rays of the sun.

No doubt many gentlemen are in possession of facts that would in some degree ascertain how much faster the boles of trees swell that stand exposed to receive the full benefit of the warmth of the sun, than those that are either partially or constantly in the shade. To make these facts known would materially benefit planters; for I am fully persuaded that there are but few persons apprised of the magnitude of the power of the sun's rays upon the boles of trees in causing them to swell.

Of the most profitable Length of Boles of Trees.

We rarely see timber trees pruned, and still more rarely do we see the pruning performed in a judicious manner. This business should commence early, never suffering the branches on the intended stem or bole to grow to a large size, in order that, when cut off, the wounds may be small and soon healed. Those who want directions for performing the operation, may think it well to consult Mr. Pontey's Forest Pruner. There are, however, divers opinions as to the most profitable height to which trees ought to be pruned, and the instruments most proper for pruning; some persons objecting to the use of the saw, unless afterwards smoothed by the knife; and not a few objecting to pruning in any way; the consequence of which is, that we often find trees that stand single, particularly oaks, with boles not more than six or eight feet high, but with wide spreading bushy tops, fit only for the fire. The shade and drip of one such tree is sometimes found to do more injury than four well-trained trees, and perhaps it is not of half the value of one of them. On the contrary, trees in close plantations are often suffered to stand so much too thick as to destroy each others branches, excepting only a few small ones near their tops; and not unfrequently we see tall elms trimmed up to within a few feet of their summits—it is certain that such trees must swell very slowly in their boles; for we find in woods where the trees are all of the same age, that those with the largest tops have generally the thickest boles.

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* On a hot day in the middle of May I have observed the mercury in the thermometer to rise and fall from twelve to sixteen degrees, on hanging it alternately in the sunny and shady sides of the same tree, between the hours of two and five o'clock, at which time of the day the heat is generally the greatest.

There is no doubt a medium length of bole for different kinds of trees on different soils, that will be found productive of more timber, or timber of more value, than boles that are much longer or much shorter. And although we may not be able previously to decide with certainty what that exact length of bole is, in any kind of trees, on any soil, which will eventually prove most profitable, yet it is deserving of investigation, if we can thereby approach with certainty to within a few feet of the exact point. It is certainly a matter of too much importance to be left, as it generally is, to each individual woodman to decide upon, according to his own vague opinion. I shall, therefore, take the liberty of stating by what steps I have endeavoured to approximate towards the most profitable lengths of boles of trees of different rates of growth, that are not intended to stand beyond the age of sixty years.

In the preceding tables the trees are supposed to be measured to the top of the leading shoot, but in the following tables only to the height of their boles of 24, 32, and 40 feet.

Tables showing the Increase of Boles of Trees of different Lengths.

If a tree has increased twelve inches in height and one in circumference annually, until it is twenty-four years old, it will then be twenty-four feet high, and three inches girt at twelve feet high; and supposing that in process of time this tree be pruned up so as to leave the bole twenty-four feet high clear of branches, and that it continue increasing one inch in circumference annually, the rate per cent. per annum of its increase will be as under, exclusive of the increase of timber in its top and lateral branches.

TABLE X.

Years old.	Girt	Contents.			Years old.	Girt.	Contents.			One Year's Increase.			Increase per Cent. per Ann.
	in.	ft.	in.	pt		in.	ft.	in.	pt.	ft.	in.	p	
24	3	1	0	0	25	3¼	1	9	1	0	3	1	17·1
28	4	2	8	0	29	4¼	3	0	1	0	4	1	12·7
32	5	4	2	0	33	5¼	4	7	1	0	5	1	10·1
36	6	6	0	0	37	6¼	6	6	1	0	6	1	8·4
40	7	8	2	0	41	7¼	8	9	1	0	7	1	7·2
44	8	10	8	0	45	8¼	11	4	1	0	8	1	6·3
48	9	13	6	0	49	9¼	14	3	1	0	9	1	5·6
52	10	16	8	0	53	10¼	17	6	1	0	10	1	5·04
56	11	20	2	0	57	11¼	21	1	1	0	11	1	4·5
60	12	24	0	0	61	12¼	25	0	1	1	0	1	4·1
64	13	28	2	0	65	13¼	29	3	1	1	1	1	3·8
68	14	32	8	0	69	14¼	33	10	1	1	2	1	3·5
72	15	37	6	0	73	15¼	38	9	1	1	3	1	3·3
76	16	42	8	0	77	16¼	44	0	1	1	4	1	3·1
80	17	48	2	0	81	17¼	49	7	1	1	5	1	2·9
84	18	54	0	0	85	18¼	55	6	1	1	6	1	2·7
88	19	60	2	0	89	19¼	61	9	1	1	7	1	2·6
92	20	66	8	0	93	20¼	68	4	1	1	8	1	2·5
96	21	73	6	0	97	21¼	75	3	1	1	9	1	2·3
100	22	80	8	0	101	22¼	82	6	1	1	10	1	2·2
120	27	121	6	0	121	27¼	123	9	1	2	3	1	1·8
140	32	170	8	0	141	32¼	173	4	1	2	8	1	1·5
160	37	228	2	0	161	37¼	231	3	1	3	1	1	1·3

But if a tree increase 12 inches in height and one inch in circumference annually, until it be 32 feet high, and in process of time the bole be pruned up to that height, the rate per cent. per annum of the increase of this bole will be as under, exclusive of the increase in its top and lateral branches.

TABLE XI.

Years old.	Girt.	Contents.			Years old.	Girt.	Contents.			One Year's Increase.			Increase per Cent. per Ann.
	inch.	ft.	in	pt.		inch	ft.	in.	pt.	ft.	in.	pt.	
32	4	3	6	8	33	4¼	4	0	2	0	5	6	12·9
36	5	5	6	8	37	5¼	6	1	6	0	6	10	10·25
40	6	8	0	0	41	6¼	8	8	2	0	8	2	8·5
44	7	10	10	8	45	7¼	11	8	2	0	9	6	7·3
48	8	14	2	8	49	8¼	15	1	6	0	10	10	6·3
52	9	18	0	0	53	9¼	19	0	2	1	0	2	5·6
56	10	22	2	8	57	10¼	23	4	2	1	1	6	5·06
60	11	26	10	8	61	11¼	28	1	6	1	2	10	4·59
64	12	32	0	0	65	12¼	33	4	2	1	4	2	4·2
68	13	37	6	8	69	13¼	39	0	2	1	5	6	3·88
72	14	43	6	8	73	14¼	45	1	6	1	6	10	3·6
76	15	50	0	0	77	15¼	51	8	2	1	8	2	3·36
80	16	56	10	8	81	16¼	58	8	2	1	9	6	3·1
100	21	98	0	0	101	21¼	100	4	2	2	4	2	2·39
120	26	150	2	8	121	26¼	153	1	5	2	10	9	1·92

But if a tree increase 12 inches in height and one inch in circumference annually, until it be 40 feet high, and in process of time the bole be pruned up to that height, the rate per cent. per annum of the increase of this bole will be as under, exclusive of the increase in its top and lateral branches.

TABLE XII.

Years old.	Girt.	Contents.			Years old.	Girt.	Contents.			One Year's Increase.			Increase per Cent. per Ann.
	inch.	ft.	in.	pt		inch.	ft.	in.	pt.	ft.	in.	pt	
40	5	6	11	4	41	5¼	7	7	10	0	8	6	10·2
44	6	10	0	0	45	6¼	10	10	2	0	10	2	8·47
48	7	13	7	4	49	7¼	14	7	2	0	11	10	7·2
52	8	17	9	4	53	8¼	18	10	10	1	1	6	6·3
56	9	22	6	0	57	9¼	23	9	2	1	3	2	5·6
60	10	27	9	4	61	10¼	29	2	2	1	4	10	5·05
64	11	33	7	4	65	11¼	35	1	10	1	6	6	4·58
68	12	40	0	0	69	12¼	41	8	2	1	8	2	4·2
72	13	46	11	4	73	13¼	48	9	2	1	9	10	3·87
76	14	54	5	4	77	14¼	56	4	10	1	11	6	3·59
80	15	62	6	0	81	15¼	64	7	2	2	1	2	3·35
100	20	111	1	4	101	20¼	113	10	10	2	9	6	2·51
120	25	173	7	4	121	25¼	177	1	2	3	5	10	2·00

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Observations respecting Trees of different Lengths in the Bole.

Trees that increase annually 12 inches in height and one in circumference, and have boles of different lengths, these boles, if of the under-mentioned lengths, increase after the rate of five per cent. per annum at the ages and heights under mentioned, and they measure as under, viz.

			Years old.		In.		Ft.		Contents.		
									Ft.	in.	p.
Trees with	12	feet boles at	46	their girt	10	at	6	high,	8	4	0
Do.	16	do.	48	do.	10	at	8	do.	11	1	4
Do.	24	do.	52	do.	10	at	12	do.	16	8	0
Do.	32	do.	56	do.	10	at	16	do.	22	2	8
Do.	40	do.	60	do.	10	at	20	do.	27	2	4
Do.	48	do.	64	do.	10	at	24	do.	32	4	0

Whatever the lengths of the boles of trees increasing as above may be, the increase is 5 per cent. per annum one year after their girt in the middle is ten inches, but not longer.

But supposing that these trees have grown to 60 years of age, and increased as above mentioned, their girt and contents at that age would be as under, viz.

							Contents.		
							Ft.	in.	p.
Trees with	16	feet boles,	13	inches girt at	8	feet high,	18	9	4
Do.	20	do.	12½	do.	10	do.	21	8	5
Do.	24	do.	12	do.	12	do.	24	0	4
Do.	32	do.	11	do.	16	do.	26	10	8
Do.	40	do.	10	do.	20	do.	27	9	4

This table shows that the advantage to be gained by pruning trees higher than 32 feet, is not an object worthy of consideration, if the trees are to be cut down at the age of 60 years.

And if it should be found that the higher a tree is pruned the slower it swells in the bole, perhaps a 24 feet bole may measure as much at 60 years old as a 32 feet bole. If it increases half an inch in girt in the last 36 years more than the 32 feet bole increases in the same time, it will very nearly equal it in measure.

A 32 feet bole with a top from 20 to 30 feet high, with many large lateral branches, is certainly a much finer object than a forty feet bole with a top only twenty feet high, with few and small lateral branches: and at sixty years old, the former will have had to increase in the last twenty-eight years, only one quarter of an inch in girt, more than the latter, to exceed it in measure, to say nothing of the excess of timber in the larger top and branches. It must, however, be remarked, that at eighty years of age, the forty feet bole will exceed the thirty-two feet bole nearly six feet; and at one hundred years, thirteen feet, provided it swell equally fast in thickness. But unless the trees be oak, fit for the use of the navy, for which an increased price can be had, I imagine few gentlemen would now choose to let their trees stand to eighty years of age, when the increase of their boles will not be four per cent.; still fewer would let them stand to one hundred, when the increase will not be three per cent. per annum.

Again, let it be supposed that trees sixty years of age have increased annually, during their growth, fifteen inches in height, and one inch and a half in circumference, the girt and contents of their boles, if of the under-mentioned lengths, will be as under, viz.

								Contents.		
								Ft.	in.	pt.
Trees with	20	feet boles,	will be 19½	inches girt at	10	feet high,		52	9	9
Do.	25	do.	18¾	do.	12½	do.		61	0	5
Do.	30	do.	18	do.	15	do.		67	6	0
Do.	40	do.	16½	do.	20	do.		75	7	6
Do.	50	do.	15	do.	25	do.		78	1	6

Taking it for granted that the shorter boles will increase faster in thickness than the longer ones, it is reasonable to expect that the forty feet bole will contain more timber than the fifty feet bole when they are both sixty years old; and if they are both sold at the same rate per foot, the forty feet bole must consequently be more valuable. If, however, a higher price can be had for longer boles, this may compensate not only for their deficiency in measure at sixty years of age, but also for their standing beyond the period when they cease paying the common rate of interest for the money they are worth, which I suppose is frequently the case as to tall elm trees, fit for keel pieces, and perhaps beech for ship planking. It is hence evident, that where the soil is such as will enable trees to grow to a great height, it will be necessary, before we decide how high to prune them, to consider to what purposes the timber can be most advantageously appropriated.

Whatever the lengths of the boles of trees increasing as above may be, their increase is five per cent. per annum, one year after their girt in the middle is 15 inches, but not longer.

Again, let it be supposed that trees sixty years of age have increased annually, during their growth, eighteen inches in height, and two inches in circumference, the girt and contents of their boles, if of the under-mentioned lengths, will be as under, viz.

								Contents.		
								Ft.	in.	pt.
Trees with	24	feet boles, will be	26	inches girt at	12	feet high,		112	8	0
Do.	30	do.	25	do.	15	do.		130	2	6
Do.	36	do.	24	do.	18	do.		144	0	0
Do.	48	do.	22	do.	24	do.		161	4	6
Do.	60	do.	20	do.	30	do.		166	8	0

Here again we may suppose that the forty-eight feet bole, by swelling faster than the sixty feet bole, may exceed it in measure at sixty years of age, and this it would do, were the girt increased only half an inch. And if the thirty-six feet bole was increased two inches in girt, it would exceed both the forty-eight and sixty feet boles. But trees of such swift growth are frequently cut down before they are sixty years old. At forty years of age the thirty-six feet bole, if it swell no faster than the forty-eight feet bole, will contain more timber if measured according to the present erroneous method. (The greater disproportion there is between the two ends of a piece of timber, and the more disadvantageously it measures, when the girt is taken in the middle.) I suppose that in timber of this swift growth, the longer boles are frequently not worth more per foot than the shorter boles; therefore, in this case, that length of bole should be fixed on which is likely to measure most at the period when the trees are intended to be felled.

Whatever the lengths of the boles of trees increasing as above may be, their increase is five per cent. per annum, one year after their girt in the middle is 20 inches, but not longer.

It appears from the last observations and calculations, that the annual increase in the boles of trees by their growth, ceases to be equal to five per cent. per annum some time between forty-six and sixty years of age, according as the boles are shorter or longer.

But it being generally allowed that oak trees, of a size fit for the navy, require to grow from eighty to one hundred and fifty years, according to the quality of the soil, and it is so stated in the eleventh report of the commissioners appointed to inquire into the state and condition of the woods, forests, and land revenues of the crown; I have therefore been calculating tables, showing what the proportionably advanced prices should be, at different periods, up to one hundred and fifty years, to pay the proprietors for letting their trees stand to those periods. These prices, especially at the later periods, very greatly exceed any that have ever been given. It certainly has been much the interest of the growers of oak timber to fell it at about sixty years of age, even if they replant the same ground. To let it stand to one hundred and twenty years of age, and sell it at the present prices, their loss would exceed double the whole value of the timber at sixty years of age. Nothing short of a sufficient price will long command a sufficient supply. Owing to too low prices, the quantity of large timber on private estates has long been rapidly decreasing; and it will be too late to commence offering reasonable prices for it when it is all gone, and no oaks left of greater growth than sixty years. To have to wait their growing the second sixty years, may bring upon us evils exceeding all calculation.

Valuations made in October, 1807, of several Plantations in Staffordshire.

The valuations were made of the trees growing within the space of a chain square, being the tenth part of an acre, of the medium growth of each plantation.

In the plantation by the mill wall there are now growing within twenty-two yards square, as under, viz.

	£.	s.	d.		£.	s.	d.
70 oak trees, containing 175 feet, at 2*s.* 3*d.*	19	13	9				
1200 of oak bark, at 12*s.*	7	4	0				
	26	17	9	or, per acre,	268	17	6

The above is part of about four acres planted in 1775, on a strong loamy soil, worth about 20*s.* an acre.

One pound per annum forborne 32 years, and improved at five per cent. compound interest, would amount to 75*l.* 6*s.*

But the value of the timber is more than three times this amount.

The ground was prepared for planting by ploughing.

On the east side of Cottage Wood there are now growing, within twenty-two yards square, as under, viz.

	£.	s.	d.		£.	s.	d.
50 ashes, containing 300 feet, at 1*s.* 6*d.* - - -	22	10	0				
13 oaks do. 7 do. 2*s.*	0	14	0				
Bark - - -	0	7	0				
	23	11	0	or, per acre,	235	10	0

The above is part of about two acres planted in 1776, partly on heaps of earth in clay pits, and partly on strong soil upon a deep bed of sand, value about 15*s.* an acre.

Fifteen shillings per annum, forborne 31 years, and improved at five per cent. compound interest, would amount to - - - - - - - 53*l.* 0*s.* 0*d.*

But the value of the timber is more than four times this amount.

In the clay pits only holes were dug for the plants, but the other part wholly trenched, or double dug with the spade.

In Pickmore Pool plantation there are now growing within twenty-two yards square, as under, viz.

97 Scotch firs, containing 636 feet*, at 1*s.*—31*l.* 16*s.* 0*d.* or, per acre, 318*l.* 0*s.* 0*d.*

The last plantation is part of about six acres planted in the springs of 1778 and-9. Much of the soil is a tough peat on gravel or hungry white sand, worth, say 5*s.* per acre.

This ground lay between two tenants who had never cultivated it. They had then nineteen years unexpired of their lease of thirty-one years of this and the adjoining lands, and willingly gave it up to be planted, on condition of having the fences made and kept in good repair.

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* This produce is after the rate of 6360 feet an acre, which is about the rate of Table IV.

Five shillings a year, forborne 29 years, and improved at five per cent. compound interest, would amount to 15*l.* 11*s.* 0*d.* But the value of the timber is more than twenty times this amount.

The trees were about two feet high, and planted at two yards distance, in holes dug with the spade, 1210 on an acre. Labour of making the holes and planting the trees cost 1*l.* 6*s.* 10½*d.* per acre.

About 2700 were planted on an acre in the other plantations, where the ground was wholly broken up.

In the remarks on these three plantations, no notice is taken of the thinnings. I am informed by gentlemen who have kept accounts of thinnings, that these have repaid the rent of the land and every expense, with compound interest, some time before the woods were thirty years old; and the preceding calculations show that it may be so. And if so, the present value of these plantations is all clear gain.

The valuer of these plantations has bought a good deal of wood out of them; and the prices he has valued at per foot, may possibly be a fair value there for such small timber.

The growth of the firs in the last-mentioned plantation, is probably as great in that poor ground as it would have been had they been planted on ground of three or four times its value; this must be a powerful inducement to gentlemen to plant all such poor ground in the first instance.

And a few of oaks, ashes, and firs, may be raised on almost every farm in screens, that may, by their shelter, increase the value of the farm to the occupier, by increasing the produce, particularly that of grass grounds. In this case the interest of landlord and tenant may be reciprocal; but it is the reverse where trees are planted in hedge-rows.

And even the sides and tops of high mountains may be made abundantly more productive of grass, if certain portions of them were surrounded by plantations. These plantations, by breaking the force of cold winds, diminish their chilling effect on the fields the plantations surround, and

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render the climate on mountains much more mild and genial.

This last kind of improvement will generally be found very greatly to exceed the expectation of the improver, provided it be judiciously planned and executed.

May I take the liberty to suggest, that information of very great value might be obtained by the Society from the gentlemen to whom medals and premiums have been given for planting trees, if they would favour the Society with their subsequent observations respecting those plantations.

It would, for instance, be desirable to have the nature of the soil and under strata described,—the sorts of trees best suited thereto,—the distance at which the trees were first planted,—at what periods they were thinned, and how many cut out at each thinning, and their measure and value,—the present height, distance, measure, and value, of the trees now growing on an acre,—what distances are found most advantageous, and also the best height to be pruned.

The fund of information that such communications might afford, would prove of very great value indeed to future planters, as well as to many proprietors of plantations of different ages now growing.

If the Society should think it advisable to solicit this information, no doubt the ample and valuable materials they would thereby obtain, would enable some abler pen to do justice to the curious and important subjects of the preceding pages. In the mean time it is hoped, that this attempt to reduce our knowledge of the growth of timber to something like system, however imperfect it may be, will be received with indulgence. Assuredly, it had not so soon seen the light, had not the present situation of our country imperiously demanded of every individual his utmost exertion to render us as independent as possible of supplies of every kind from the continent of Europe, from which we are now almost totally excluded.

C. Waistell.

High Holborn, March 15, 1808.
To Charles Taylor, M.D. Sec.

LXVI. On

LXVI. *On the intended Thames Archway between Rotherhithe and Limehouse.* *By Mr.* John Farey, *Mineralogical Surveyor.*

To Mr. Tilloch,—Sir,

In an age like the present, when the abilities of a *Rennie*, a *Jessop*, a *Telford*, and numerous other British civil engineers are so universally known, by the great works which have been executed within the last 30 years under their direction, wherein difficulties of almost every kind have been successfully overcome, and tunnels in the most difficult situations have been constructed, in considerable numbers: it must excite surprise in every one to learn, that after more than three years have been spent by a company of proprietors in the metropolis of the country, in ineffectual attempts towards constructing a dry tunnel for a road-way under the bed of the river Thames, as a substitute for a bridge, that the directors appointed by these proprietors should now be *advertising* (in the newspapers, and by a printed hand-bill, which is given below,) for the schemes of inexperienced adventurers, rather than call in the professional aid of one or more of the established engineers of the country, to the effecting of the purposes which they have in view. Surely it cannot be expected by these gentlemen, that any of the experienced engineers alluded to, will submit their designs and estimates for the great work which the proprietors have undertaken, on the terms and for the considerations held out;—*who* is to decide on the merits of the different designs which may be delivered in? and *who* is to superintend and execute the design which may be adopted? On both of these questions, the probability either of honour or profit to be derived from their labours, will in all likelihood turn, according to the conditions which the directors have laid down.

In your Magazine, No. 97, for June 1806, (vol. xxv. p. 46,) I gave a hasty sketch of the state of the works at that time, and an account, extracted from Mr. *Robert Vazie's* books, of the *strata* expected by him to be met with in sinking the shafts on the south and north shores, and in driving under the

A Section and Plan of the proposed Thames Archway, shewing the drift or heading which has been constructed under the River.

Fig. 1.

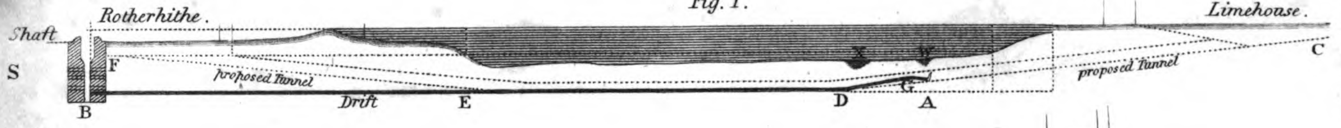
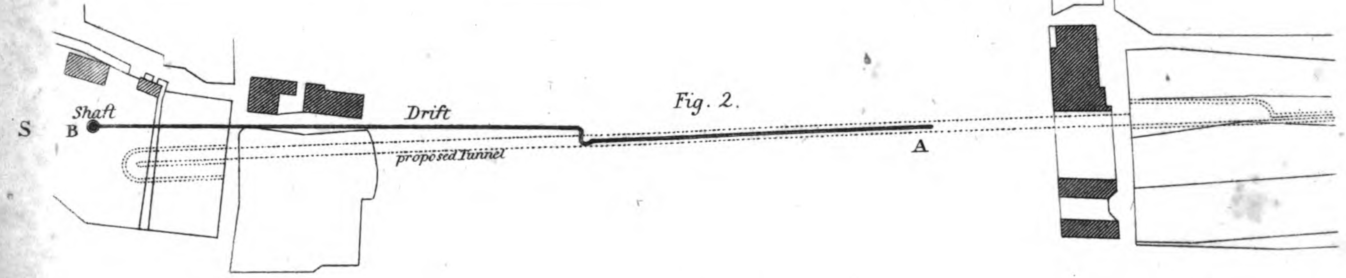
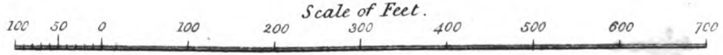


Fig. 2.





S. Porter, sculp.

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the river between these shafts: at that time Mr. V. was very confident in considering the strata as *regular and undisturbed* which his borings had penetrated, notwithstanding my opinion expressed to him on seeing the specimens, that the whole were *alluvial*, and their continuance horizontally not in the least to be depended on, as mentioned at page 49 of my paper in your Magazine, of which he had three or four copies from me at his request, to distribute among the directors of the concern. The following accounts, which the directors have lately published, show, after more than two years of very expensive trial, that these ideas of mine have been confirmed, as they might have been in a few weeks time, by the *borings* in the bottom of the river which I recommended, first in *Dr. Rees's New Cyclopædia* (sect. *Thames*, in the article CANAL), and again in my paper in your Magazine above referred to; and such borings would doubtless have suggested the conclusion, without all this loss of money and time, which the *engineer* (Quere, was it Mr. V. or Mr. T.?) at length came to, viz.: "That an *underground* tunnel could not be made in that line, unless the fractures were covered by caissons, without which the further progress of the drift would be useless;" but he continues, "that he had no doubt of being able to make a tunnel over the *same* line through the river, sufficiently deep into its bed, by means of movable caissons, or coffer-dams, and at a less expense considerably than the original estimate for the underground plan: and *without any impediment to the navigation of the river*." From the expressions of the directors which follow, it is too much to be feared, that the counsels which first prevailed in adopting a deep *underground* tunnel, rather than one laid as near as may be to the bottom of the water in the river, has still a prevailing influence among them; in which case I venture again to predict, that the expectations of the proprietors and the public, will be ultimately and grievously disappointed.

At the time of writing the short notice of this undertaking, in connection with the navigation of the *Thames* river, for the Cyclopædia, I was too much hurried to give the subject that consideration, which its obvious importance

has

has since occasioned it to have, particularly when I have been in the company of ingenious and practical men in such matters, and have introduced the subject, in order to hear the ideas of others relating to it. Instead of *piling off* the river, as I there hastily mentioned, I would suggest to the consideration of those engineers who may turn their attention to the subject, an immense tub in form of the frustum of a cone, secured by hoops outside, and polygonal framing inside, but without either bottom or top, which being principally of wood, might be floated at high water to the spot where the tunnel is to be begun; the bottom edge of it might be so secured to the bottom of the river, by means which will suggest themselves to competent engineers, as to prevent the influx of water from the river, or the pressing in of quick-sand or silt, after the water is extracted from the tub by engines; and after a length, of 70 or 80 feet perhaps, of the tunnel was constructed, its end might be so secured as to admit of moving the tub, to include a new length; with scarcely any interruption to the navigation of the river.

I have often been sanguine enough, to expect to see several tunnels constructed under the Thames and other of our important navigable rivers, in some instances superseding, perhaps, the ancient bridges like that of London; but must confess, that the origin and proceedings of the two Thames Archway Companies, which we have seen, and particularly the proposals by the directors of one of them, which follow, have occasioned me to doubt much, whether I shall live long enough, to pass through a tunnel constructed under their auspices, unless indeed a material change takes place in their principle of proceeding.

I am, sir, your obedient servant,
JOHN FAREY.

Westminster, May 12, 1809.

Particulars of the *strata* met with, in sinking a shaft near the Horse-Ferry in Rotherhithe, and in driving a heading thence under the bed of the Thames river, 1035½ feet in length, intended as a drain for the proposed road archway: with an account of the progress and present state of the works, and of two premiums advertised by the directors for plans, according to which the tunnel may be constructed.

The Thames Archway Company was established by an act of parliament (45 Geo. III.) "for the purpose of forming a tunnel under the river Thames, either for foot passengers or carriages, or for both;" and by the unanimous opinion of every engineer who had been consulted, it was deemed necessary, as a preparatory step, to make a drift-way to extend as far as the deepest part of the river; and according to the original plan of this undertaking it was intended then to begin to construct the tunnel, carrying it forward in both directions from the centre to the north and south sides of the river: a shaft was therefore sunk on the south side near the Horse-ferry, Redriffe, and a drift-way made to the point first proposed. It was, however, then determined to continue the drift to the opposite shore, in the line and direction of the proposed tunnel, for the sake, amongst other reasons, of exploring the ground through which that part of the tunnel was intended to pass; and thereby enabling the engineer to anticipate and guard against difficulties.

In pursuance of this determination the drift-way was carried on to the extent shown in the accompanying plan, at A, when the engineer proposed another mode of executing the tunnel, and, in his opinion, much less difficult and less expensive, and for which the further extension of the drift would be useless; the directors, being convinced that there are many methods of accomplishing the object, and that it is their duty to procure the best in their power, thought proper, before this or any plan were adopted, to suspend the works, and to invite ingenious men of every description to a consideration of the best means of completing so useful and so novel an undertaking.

With this view the directors are induced to offer the following premiums, namely,

Two hundred pounds to the person whose plan shall be adopted and acted upon; and a further sum of *three hundred pounds* if it be executed.

The first premium to be paid within three months after the plan shall have been put in execution. The second premium within three months after the tunnel shall have been opened for passengers.

The plans to be accompanied with full and clear specifications and directions how to carry them into execution, and an estimate of the expense. They must be signed by fictitious names, mottoes, or marks, and will be returned if not adopted to any person claiming them under the fictitious name, motto, or mark; the real name to be enclosed in a sealed note, and externally marked with the fictitious name, &c.; which note shall not be opened unless it be that of the person whose plan shall be adjudged entitled to the premium.

All the plans will be submitted to the judgement of eminent and competent persons chosen by the directors, who shall not be either proprietors or competitors; so that every person offering plans may rely upon the fairness and impartiality of the decision.

The plans must be delivered at the office of Mr. Wadeson, in Austin Friars, London, solicitor to this concern, on or before the first of June next.

To enable engineers and others to form correct opinions on the subject, the directors have caused the following account of such facts as were noted to have occurred in the progress of the undertaking, to be extracted from the engineer's journal, which is accompanied by an engraving showing the plan and section of the works as far as they have proceeded.

Fig. 1, (Pl. XI.) is the section of the river, shaft, and drift-way, B the shaft on the south shore lined with nine-inch brick-work laid in cement impervious to water. The strata through which it passed consisted of,

	Ft.	In.
1. Brown clay	9	0
2. Loose gravel with a large quantity of water	26	8
3. Blue alluvial earth inclining to clay	3	0
4. Loam	5	1
5. Blue alluvial earth inclining to clay, mixed with shells	3	9

6. Cal-

	Ft.	In.	Ft.	In.
6. Calcareous rock in which are imbedded gravel stones, and so hard as to resist the pick-axe, and to be broken only by wedges	7	6		
7. Light coloured muddy shale, in which were imbedded pyrites and calcareous stones	4	6		
8. Green sand with gravel and a little water	0	6		
9. Green sand	8	4		
			68	4
From the surface of the ground to high water mark			8	0
Depth of the shaft from high water mark			76	4

The gravelly stratum No. 2, in the shaft extends about 400 feet into the river from high water mark at T to V; at this latter place it is about two feet thick, and underneath is alluvial earth approaching the nature of clay.

The framing of the drift consists of three-inch plank, five feet high, three feet wide at bottom, and two feet six inches at the top inside.

Fig. 2 is a plan of the drift-way and shaft.

In proceeding with the drift-way from the south to the north shore, the strata were constantly varying at the face of the drift as noted at the following places specified. The variations in the intermediate spaces were not noted.

Face of the drift at the entrance from the shaft, measuring from the bottom upwards,	Ft.	In.	Ft.	In.
Green sand	4	6		
Gravel	0	6		
			5	0
At 177 feet from the shaft, Green sand	4	0		
Gravel	0	6		
Blue muddy shale	0	6		
			5	0
At 234 feet, Green sand	3	9		
Gravel	0	3		
Blue muddy shale	1	0		
			5	0

At

		Ft.	In.	Ft.	In.
At 295 feet,	Green sand	3	7		
	Gravel	0	3		
	Blue muddy shale	1	2		
				5	0
At 317 feet,	Green sand	3	5		
	Gravel	0	4		
	Blue muddy shale	1	3		
				5	0
At 321 feet,	Green sand	3	3		
	Gravel	0	4		
	Blue muddy shale	1	5		
				5	0
At 333 feet,	Green sand	3	3		
	Gravel	0	4		
	Blue muddy shale	1	5		
				5	0
At 350 feet,	Green sand	2	8		
	Gravel	0	4		
	Blue muddy shale	2	0		
				5	0
At 493 feet,	the green sand ends.				
At 730 feet,	Hard calcareous rock, mixed with loamy sand			5	0
At 799 feet,	Hard rock			5	0
At 858 feet,	Ditto			5	0
At 901 feet,	Ditto			5	0
At 931 feet,	Rock, with a little sand and shells, and water in the roof			5	0
At 945 feet,	Hard rock	2	6		
	Clay and shells	2	6		
				5	0
At 966 feet,	Rock	0	3		
	Clay	0	4		
	Shells	2	0		
	Clay	1	0		
	Cockle shells	0	4		
	Clays and shells	1	0		
	Sand	0	2		
	Clay	0	6		
	Sand	0	5		
				6	0
At 972 feet,	Clay and shells	4	0		
	Sand	1	0		
				5	0

At

At 992 feet,	Clay and shells	0	8	
	Sand	4	4	
				5 0
At 1011 feet,	Sand	3	6	
	Clay	1	6	
				5 0

The quantity of water in the gravelly stratum No. 2 of the shaft, was so considerable, that a fourteen-horse engine could only keep the water a few feet below its natural level, and the shaft was sunk through, by far the greatest part of this stratum, into the blue stratum No. 3, with the water standing in it to the depth of several feet. It is well ascertained that this stratum of gravel extends through a considerable part of the adjoining country; but borings being made in the shaft from the bottom of this stratum, no water was met with in the sub-strata to the depth of eighty-six feet from high water, where a spring was discovered, which rose in a few hours, through pipes inserted for that purpose, to a higher level than that in the gravelly stratum No. 2. The shaft was therefore sunk only to the depth of seventy-six feet four inches.

The drift was then carried forward in a horizontal direction to the north, five hundred and fifty-nine feet. And, in order to explore the ground in the northern part of the line of the then proposed tunnel, the drift was turned to the west twenty-three feet six inches from the centre of the former line to the centre of the new direction, and then to the north, as shown in Fig. 2, (intended to be enlarged afterwards to the size of the tunnel) and carried forward three hundred and forty-one feet, making the distance from the shaft to the beginning of the rise at D nine hundred and twenty-two feet. Through the whole of this line no material interruption occurred; the strata, as shown above, consisted of firm sand, calcareous rock, and concreted gravel, with no more water than was easily kept under by a fourteen-horse engine.

At the point D the drift was made to incline upwards at the rate of one foot in nine. In prosecuting this part of the drift, at the distance of twenty-three feet from the beginning of the incline, the earth in the roof broke down, and discharged a great quantity of sand and water into the drift. At the time this circumstance happened, a space of only six inches by thirty of earth in the roof and none in the face was left untimbered; and through this space the earth kept falling by degrees, until a hole was formed capable of letting a man stand up in it; who observed a quicksand, about three feet thick, and about four or five feet above the roof of the drift. The stratum between the drift and sand was clay; water flowed from the sand. The hole was after some difficulties filled up, and the works proceeded.

From the observations which had been made in the progress of the drift, the engineer found that the strata dipped slightly from the south to the north, and concluded that the gravelly stratum No. 2 in the shaft would end in quicksand. This inference was confirmed by borings in the north shore at E, and by the fact that the wells there are much deeper than on the south. In expectation therefore of drawing off the water from the face of the work, borings were made at D, through the roof of the drift, and pipes forced up to the top of the quick-sand, which had the desired effect. The water came free from sand for a considerable time; but when the sand began to come through any of the pipes they were plugged up, and others occasionally inserted in different places to the south of these, with the same object in view; and which kept the face of the work dry. By this means, and by using the utmost precaution in all other respects, the drift was afterwards extended seventy feet beyond this fracture; where the roof broke down a second time, and sand and water entered the drift-way with great violence, and to an alarming degree; so that in about a quarter of an hour the water rose in the shaft nearly to the top of it. On examining the river an opening or hole at *w* was discovered in the bed, of about four feet diameter and nine feet deep, and its sides nearly perpendicular. Into this hole, clay partly in bags, and other materials, were thrown sufficient to fill it up; and which succeeded in stopping the communication between the river and the drift. The face of the drift was again opened; but the men could make but little progress, as the water and sand frequently burst in upon them, and drove them away. Pipes were again put up at G, and the drift was extended twenty feet six inches further, in nearly a horizontal direction, through the quicksand. The face was then timbered up, to prevent any further fall of earth or sand; and a pipe nine feet long forced upwards diagonally at the face of the drift. The first eight feet through which this pipe passed was blue clay, and the last foot quicksand, of which a considerable quantity immediately flowed into the drift. This pipe soon became clogged up, it is presumed with clay, as some lumps came through nearly as large as the diameter of the pipe. Another pipe, eight feet six inches long, was inserted horizontally in the face, and discovered nothing but blue clay: no sand nor water came through it.

At this period the engineer reported, that he had examined the bed of the river, and found the hole at *w* considerably increased both in width and depth, and the earth at *x* very much sunk; and that he had no doubt these two fractures communicated underneath. He then gave it as *his opinion* that an *underground* tunnel could not be made in that line, unless the fractures were covered by caissons, without which the further progress of the drift would be useless; but that he had no doubt of being able to make a tunnel over the *same* line through the river, sufficiently deep into its bed, by means of moveable caissons, or coffer-dams, and at a less expense considerably than the original estimate for the underground plan; and *without any impediment to the navigation of the river.* Under these circumstances the further progress of the works was suspended. But the directors think it necessary to state, that although the engineer then in the Company's service was of opinion that an underground plan could not be executed in or very near the proposed line, yet there are others of a contrary sentiment; and notwithstanding the directors are in possession of designs or plans (which may be inspected on application at the clerk's office in Austin Friars) for completing the undertaking, yet wishing to avail themselves of all the ability of their country, in an undertaking of such novelty and importance, it becomes their duty to await the event of this address to the public, before any plan be adopted, however considerable its merits, or however eminent its authors.

In the design of any plan for this concern, engineers will doubtless pay particular attention to the difficulties which are likely to occur, from the situation of the quicksands, the communication with them and the river, and the falls in the bed of the river. And that they will not consider themselves as prevented from offering plans for executing the tunnel through the river itself, by means of caissons, coffer-dams, or any other method (if such method appear to them preferable to the underground mode), provided in the execution of such plans *no impediment be occasioned to the navigation of the river.*

It is necessary to state, that any alteration in the line of the tunnel can be but inconsiderable, as it must be confined within the limits of the ground laid down in the accompanying plan.

It is an important consideration with the Company, that the size of the tunnel be large enough to admit two carriages to pass each other; or two of smaller dimensions, each to admit a carriage.

The Company contemplate a foot tunnel, only in the event that a larger one should appear to be impracticable.

The plans must be formed with regard to the tunnel being lighted.

N.B. That plan whose line is the shortest, and ascent the easiest, will have great claims to preference, if equal in merit in other respects.

Reference to the Plate.

Fig. 1, (Plate XI.) section of the river and works. Fig. 2, plan of the same.

B the shaft. A B the drift-way, as far as it has been executed. The dotted lines D *e* and E F, Fig. 1, show the proposed ascent and opening of the tunnel. The dotted lines in the plan, Fig. 2, show the proposed direction of the tunnel. The width of the river at low water is 649 feet, at high water 850 feet. The distance between the drift-way and the bottom of the river between D and E is no where less than 28 feet, and from D to A no where less than 25 feet.

The parts shaded in plan Fig. 2 are buildings.

LXVII. *On the Fibres used in Micrometers: With an Account of a Method of removing the Error arising from the Inflection of Light, by employing Hollow Fibres of Glass. By* DAVID BREWSTER, *LL. D., Fellow of the Royal Society of Edinburgh, and of the Society of the Antiquaries of Scotland.*

DEAR SIR,

HAVING directed my attention for some years to the construction of micrometers, I have had frequently occasion to regret the difficulty of procuring fibres sufficiently fine and elastic for these delicate instruments. The impossibility of obtaining silver wire of a diameter small enough for this purpose, has induced Mr. Troughton to use the web of the spider, which he has found so fine, opaque, and elastic, as to answer all the objects of practical astronomy. I am informed, however, by this celebrated artist, that it is only the stretcher or the long line which supports the spider's web, that possesses these valuable properties. The other parts of the web, though equally fine and elastic, are very transparent, and therefore completely unfit for micrometrical fibres. The difficulty of procuring the proper part of the spider's web, has compelled many opticians and practical astronomers to employ the raw fibres of unwrought silk, or, what is much worse, the coarse silver wire which is manufactured in this country. But whatever be the relative advantages of these different substances, they are all liable to the error arising from the inflection of light, which renders it impossible to ascertain the exact contact between the fibre and the luminous body. This disadvantage has been experienced by every astronomical observer, and has always been considered as inseparable from the wire micrometer. I have, however, succeeded in obtaining a delicate fibre which enables us to remove the error of inflection, while it possesses the requisite properties of opacity and elasticity. This fibre is made of glass, which is so exceedingly elastic, that it can be drawn to any degree of fineness, and can always be procured and prepared with facility; a circumstance of no small importance to the practical astronomer, who is frequently obliged to send his micrometers to a great distance to be repaired.

It is evident that this vitreous fibre, when drawn from a hollow glass tube, will also be of a tubular structure, and that its interior diameter may always be regulated by the inner diameter of the original tube. When the fibre is formed, and stretched across the diaphragm of the eye-piece of a telescope, it will appear perfectly opaque, with a delicate line of light extending along its axis. This central transparency arises from the transmission of the incident light through the axis of the hollow tube: and since this tube can be made of any calibre, we can also increase or diminish the diameter of the luminous streak. In a micrometer which I have fitted up in this way, the glass fibres are about the 1200dth part of an inch in diameter; and the fringe of light which stretches across their axis is distinctly visible, though it does not exceed the 3000dth part of an inch.

In using these fibres for measuring the angle subtended by two luminous points, whether they be two stars, or the opposite extremities of a luminous disc, we may, as has been done hitherto, separate the fibres till the luminous points are in contact with their interior surfaces; but in order to avoid the error arising from inflection, I would propose to separate the fibres till the rays of light issuing from the luminous points dart through the transparent axes of the fibres. The rays thus transmitted evidently suffer no inflection in passing through the fibre to the eye; and besides this advantage we have the benefit of a delicate line about one third of the diameter of the fibre itself.

I am, dear sir, your most obedient servant,

D. Brewster.

To Mr. Tilloch.

LXVIII. *Ob-*

LXVIII. *Observations suggested by the Geological Paper of Mr.* John Farey *in last Month's Philosophical Magazine.*

To Mr. Tilloch,—Sir,

The geological facts communicated by Mr. John Farey, in his Paper commencing your last month's Number, are in an eminent degree instructive and interesting. It is only from the itinerant geologist cautiously pacing over various and extensive districts, and marking, with experienced intelligence, the wonderful phænomena which every where present themselves, that we can hope to obtain that accumulation of practical facts which can alone guide us to a sober and correct theory of the natural causes which, at remote periods, have operated those stupendous changes which are every where seen on and near the surface of our globe.

The almost infinitely diversified exterior of the earth, and its universal stratification, furnish the most interesting subjects of inquiry; and every natural inequality upon, and every bed which reposes beneath, the surface is connected with a history which well merits, and can only be developed by, the researches of the strongest intellect. Indeed the common mind is overpowered by the stunning magnitude of geological facts; it shrinks from the bold but just conclusion, that the lowest stratum which the deepest excavations into the earth have yet reached, was once itself a surface, and that the highest peak of the loftiest stratified mountain is only the remaining speck of a vast country which once spread itself out on the same, and in many instances much higher, level: the mountain deriving its present form and exaltation, not from masses of matter successively piled up by unknown means, but solely from the superior durability of its materials, which have withstood the operation of those tremendous agents, that have swept away the surrounding country in which it was imbedded, leaving the mountain itself a magnificent land gauge, by which to estimate the immensity of the tracts that have disappeared. The formation of mountains in this way, and that of the extensive strata of the earth, mutually elucidate each other. The incalculable masses of materials necessary to form the latter, could only be derived from the destructive transportation of other strata equally extensive; and the present elevation of stratified mountains is demonstrative evidence of the former existence of the countries which, in disappearing, have furnished such vast masses of diversified material for the formation of other stratified countries in other situations. These simple and sublime geological truths, however they may now shock minds unaccustomed to the contemplation of natural grandeur, will, at no distant period, be as generally assented to as the gravitation of Newton.

The just appreciation of geological phænomena is amongst the most creditable things of modern science. Already are the *ignes fatui* of hypothetical invention disappearing, and we no longer hear of seas fourteen thousand feet above the level of the present ocean retiring into cavernous receptacles, or of the exaltation of continents, to equal heights, by vulcanian energies. Forged in the same fabulous workshop, they are already slumbering on the same shelf with the vitreous sparks of Buffon.

The great source, I conceive, of all hypothetical reasoning on the formation of the earth arises from the mistaken opinion, that the present laws of nature are insufficient to account for past effects; without duly considering, that the natural causes which are now operating changes on our globe have been in action millions of years, and that it is the almost infinite duration and variance of their action, rather than the apparent little which we can now perceive them performing, that will enable us to account for the stupendous effects which they have accomplished. The system-builder by a deluge, an internal fire, an external crust, the vicinity of an erratic planet, or some such fanciful creation, is for accomplishing, almost in an instant, that which, far more probably, required many thousands of years to effect; and it assuredly is a rigid attention to, or disregard of, the two important circumstances of time and agency, that marks the boundary line between fanciful hypothesis and genuine theory. When the investigator flies off in search of a cause which no longer exists, or no longer operates within the sphere of his inquiry, he is certainly indulging in hypothetical visions; but when he fairly generalizes, by some common agreement, a multitudinous class of acknowledged phænomena; and directly connects them with causes still in operation, he is developing a lucid theory which will enlighten and improve.

It is this departure from nature, by an assumption of extinct or imaginary causes, that has induced me to offer the present observations. The closet geologist may be expected to indulge himself in the creation of hypothetical phantoms; but that he who has had the great and instructive volume of Nature spread out before him, and, page after page, has read, in her indelible and expressive characters, the history of her magnificent transactions, should imagine her present energies unequal to her past performances, and that others must be sought for in lunar regions, forsooth, is at once matter of surprise and regret.

I apply this to Mr. Farey, but with the utmost deference for his practical knowledge. In any thing relating to effects which have taken place, and to practical facts resulting from extensive personal observation, he is clear, correct, and instructive; but the instant he attempts to develop cause, the genuine spirit of philosophy forsakes him, and he becomes bewildered in the unprofitable maze of hypothesis. Mr. Farey is evidently preparing to add one more inventive system to the many that have already so much retarded the progress of real knowledge, by the introduction of a non-existent satellite at some indefinite time, and from some indefinite region, whose near approach to the earth is to reverse the action of gravitation, and undulate or distort the upper strata into some or all of their present irregularities. Before further committing himself, it will be well for Mr. Farey to consider whether, by the promulgation of a hypothesis so utterly incongruous with all the present operating laws of Nature, he is not about to sacrifice much of that fair fame which his practical researches have so deservedly assigned him.

A system-builder, like a religious or political bigot, is ever a most irritable being, and to prick his favourite bubble

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is to explode all his virulence: but I confidently trust that Mr. Farey will be found a distinguished exception to this, and that he will hail with approbation a liberal criticism which has solely in view the expulsion of error from his favourite pursuit, and the recalling of his attention within those sober limits which experience and observation so justly prescribe. A dwarf stationed on the shoulder of a giant can see further than the giant himself; and if I assume this visual preeminence, it is only to acknowledge the Colossus that supports me.

Certainly the great and most desirable desideratum in geology is to account, satisfactorily, for the original formation of all stratified countries; and when that has been clearly accomplished, all irregularities and anomalies in the strata themselves, which have hitherto been almost the only circumstances attended to, will be comparatively an easy attainment; for it is impossible to doubt that the same powerful agent, whatever it may be, that has given mobility and transportation to such massive and diversified materials, and has spread them out, on so gigantic a scale, over the face of the globe, must also be equal to their separation, disruption, denudation, excavation, and almost every other geological appearance which observation has discovered.

And I have only to advance one step further and add, that the only agent in nature, with which we are acquainted, and to whose action we can assign, with any colourable probability, all these extraordinary and stupendous effects, is *water*.

To this powerful and incessant operator allow but a sufficiency of duration, and a suitable diversity of fluctuating circumstance, and he will have a bold and arduous task to perform who shall undertake to advocate its limitation in geological efficacy. And here it is that I would more especially solicit the attention of Mr. Farey, by urging him to relinquish his aërial assistant, which does not untie, but clumsily cut, the Gordian knot, and substitute in its stead this simple and natural instrument, in which he will experience a power and pliability of action competent to the illustration of almost every geological phænomenon. It is, however, I think, very unfortunate for Mr. Farey himself, that he either does not perceive, or is strangely indisposed to admit, the most obvious effects of water on the surface of the earth. A decisive and very singular proof of this is given in that most extraordinary and unphilosophical conclusion which he draws on the formation of valleys, and which unquestionably detracts, to an extent which he cannot be aware of, from his other acknowledged merits as an observing naturalist.

The action of water, in operating extensive changes on our earth, naturally divides itself into two distinct branches; those changes which are effected by streams of fresh water running over the surface, and those far more mighty exterior and interior changes which the ocean itself has accomplished, during the submersion of our present continents.

Were there any probability, Mr. Editor, that these cursory remarks of mine could merit a place in your most respectable repository of scientific knowledge, I would pursue the subject in two subsequent papers on both of these branches; first by investigating, circumstantially, the formation of all valleys through which streams are now running, and afterwards adverting to the diversity and magnitude of marine action.

I am, sir,
your most obedient humble servant,
JOHN CARR.

Princes Street, Manchester,
May 13, 1809.

LXIX. *Introduction to the Study of Mineralogy.* By *M.* HAUY *.

IF the motives which invite us to cultivate a natural science were founded merely upon the interest which certain productions of themselves inspire, and upon what appears at first sight attractive, zoology and botany would seem to have a preponderance over mineralogy which would attract a greater number of admirers.

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* This is a translation of M. Hauy's Preliminary Discourse to his celebrated Work on Mineralogy.

Minerals, for the most part being hid within the cavities of the earth, only come out of it in fragments, and bear the marks of the iron instruments that have been employed to tear them from their beds: to the generality of mankind they are only crude masses, without character and without appropriate definition, and appear as if intended solely to be appropriated to our wants. It has seldom been imagined that a distinct science could have been reared out of the subject, and that the naturalist should hold a place between the miner who extracts the treasures of Nature from the earth, and the artist who works them.

Those however who, without dwelling upon first appearances, will consider minerals more closely, and with long continued attention, will easily perceive how much is to be gained by a more intimate acquaintance with their properties.

Polyhedric forms, the dimensions and angles of which appear to have been regulated by a scientific hand with the assistance of the compass; the variations which these forms, without ceasing to be regular, undergo in one and the same substance; and the advantage of being able, by the help of calculation and observation, to re-discover the traces of Proteus concealed under these metamorphoses; ingenious experiments concurring with indications which speak at once to the eye, in order to develop the properties which escape him; the principle of Archimedes applied to the comparison of weights under a given volume; the refrangent power employed in tracing a limit between bodies through which the image of each object appears simple, and those which present two to the astonished beholder; heat substituted for friction in order to produce electrical poles, in bodies the crystalline form of which, by particular modifications, indicates beforehand the positions of these poles; the magnetic needle making use of iron to disclose itself; various chemical agents presenting methods of dispelling doubts which other experiments had still left; the resources furnished by analysis for the formation of a method grounded upon the intimate knowledge of the objects which it embraces; every thing conspires to make mineralogy a science worthy of being received by minds naturally inclined to inquiries susceptible of precision and vigour, presenting ingenious combinations, and a collection of facts closely connected with each other.

To such minds mineralogy presents itself under a new aspect. It is a picture which is embellished by the mere habit of seeing and studying it; in which Nature exhibits herself, as she does every where else, under an aspect which claims for the Creator the tribute of our homage and admiration.

Mineralogy embraces a multitude of productions which human industry has not yet been able to mould to the wants or pleasures of life, without a certain study of their characters and of their nature, and without which art could not possibly clear the paths of science. From the earliest times the collection of these familiar productions had been subdivided into stones, salts, bitumens, and metals. The methods of the mineralogist are, as it were, the first outlines of a picture. The working of metallic substances had shown several essential differences which distinguish them. Among the stones there have been composed numerous groupes under the names of marbles and gems, which, notwithstanding the disparity of the bodies which they served to connect with each other, were attempts at the formation of the genera which subdivide the classes. Certain properties, remarkable from their being elicited under certain circumstances only, have not escaped attention: the attraction exercised by amber when rubbed over light bodies, and the kind of sympathy between iron and the magnet, which had been considered as a simple stone, have all been observed. Even the forms of crystals were not wholly unknown to the ancients: that of rock crystal and of the diamond have been distinctly alluded to by Pliny*. The regular polyhedrons, which at present excite our admiration from their multitude and diversity, were then also remarked as wonderful singularities.

It is only since the commencement of this century, however, that the learned have begun to submit the assemblage of inorganic bodies to methodical arrangements, and that the term *mineral kingdom* has been adopted. Among the various systems which have successively appeared, some of them, such as those of Linnæus, Wallerius, Daubenton, &c., employ in the determination of the species, genera, orders and classes, certain characters which are, as it were, presented to the naked eye; such as those which are derived from the form, texture, and transparency of the colours; or certain properties easily verified, such as those of emitting light with steel, effervescence with nitric acid, &c. Other systems, subjected to a more scientific progress, as traced by Cronstadt, and followed by Bergman, Born, Kirwan, &c., present the series of minerals classified according to their analyses; so that, the species being determined by the identity of the component principles, the genera are formed of species which have a common principle. The same method also serves in certain cases to connect together several genera in one and the same order: thus the neutral salts may be subdivided into alkaline salts, earthy salts, and metallic salts, according to the kind of acid united to an alkali, an earth, or a metal. But when analysis failed in enabling mineralogists to form orders, its place was supplied by some chemical property common to all the genera of which each order was the assemblage: and with respect to the classes, they were in the same way characterized after the manner in which the substances which composed them were modified in the various operations which spring from chemistry.

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* Hist. Nat. l. xxxvii. c. 2 & 4.

It must not be thought, however, that there was a line of separation clearly traced betwen the two modes of methodical distribution which we have mentioned. Chemists, after having determined the series of the classes, orders, genera, and species, by the help of chemical properties, or of the results of analysis, could not descend to the varieties, except by employing external characters in order to distinguish them from each other. Now, in a complete method, we are the less entitled to dwell upon the species, as they are frequently ramified into several subdivisions, the differences of which, much more striking than those light and fugitive shades which modify the varieties in botany, present, from the different laws of Nature, or from the different ways in which she operates, results very distinct. In calcareous species, for example, the various crystalline forms, stalactites, marbles, &c., are so many modifications of one and the same substance, which, without doubt, deserve to be separately observed and studied; and if in all these we were not to see any thing but lime and carbonic acid, it would be as if we contented ourselves with the inscription of a picture equally interesting by the assemblage and by the variety of its objects.

On the other hand, it is evident that mineralogists have really profited to a certain point, by the results of chemistry, in order to form the distributions which have been designated by the term mineralogical methods: for without speaking here of the use which they have made of certain properties, such as effervescence with the acids, which is a true chemical property, they never could have been able, without the aid of analysis, to refer subsances to their true classes. The carbonate of lead, commonly known as *white lead*, was regarded as a species foreign to the metals, and was probably arranged among the stones. In the Brisgaw, a few years ago, there was found a crystallized substance with small incrusted laminæ, and of a white colour: mineralogists had alternately regarded it as a zeolite, and as a ponderous spar. The analysis of Pelletier, however, assigned its true place, as being among the ores of zinc, by the name of calamine.

Chemistry has therefore been, at least tacitly, the guide of mineralogists in the determination of species; and the formation of the genera is really the point at which systems in every respect begin to diverge.

In those of the mineralogists, the species which compose one and the same genus are connected with each other by a character derived from some quality which is common to them, or by several characters so combined that their assemblage is considered as belonging only to the collection of the species in question. The genera adopted by chemists have their foundation in analysis itself: they depend, as we have said, upon the existence of a principle common to the different kinds, the distinction of which afterwards bears upon the principles which are peculiar to them.

We see from what precedes, that chemistry and mineralogy necessarily concur to the formation of a method, whatever it may be, which has for its object the classification of inorganic bodies; that it belongs to chemistry to lay the first foundations of the method by the determination of the species; and that the difference depends upon what is contributed by each to the construction of the edifice which is raised upon that basis. I shall soon detail the principles which seem to me to conduce to the most advantageous application of this kind of alliance.

On the other hand, natural philosophy unites with chemistry in order to furnish mineralogy with distinctive characters, the more advantageous from their diving to the very bottom of substances, and they are much less variable than those of which we judge only with respect to the manner in which they strike our senses. Experiments equally simple and easy seem to give us new organs, in order to penetrate to the most intimate properties of a substance: and we may answer those who think that mineralogy is sufficient for its own wants, without intermixing with foreign substances, that in operations so elementary, and requiring so small an expense, we see neither the naturalist nor the chemist properly so called, but the mineralogist alone, interrogating nature in a more urgent and more fortunate manner*.

Geometry, in its turn, has direct and necessary relations with mineralogy, by the description of crystalline forms, and still more by its numerous applications to the structure of crystals, which, of itself, is only the result of a natural

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* Although this simple indication of chemical and physical properties be sufficient for fulfilling our principal object, we have thought it right to add the explanation of these properties, and thus to labour for men more particularly versant in the sciences by which mineralogy may be extricated from the labyrinth of phrases purely descriptive, and be raised to the rank of the true sciences, which aggrandize their object by ascending to the laws to which they are subjected. They will of course do us a service, if they do not confine us to the results of solitary experiments, but, on the contrary, proceed to show their connection with the causes upon which they depend.

geometry,

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geometry, subjected to particular rules, and by which each solid has its figure determined by the combination of an infinity of other small solids, which are like the elements of the first. A hasty glance at crystals will obtain for them the appellation of pure *lusus naturæ ;* which is only an elegant way of confessing our ignorance. A closer examination unfolds to us the laws of arrangement in them, by the aid of which calculation represents and unites to each other the results observed ; laws so variable, and at the same time so precise and regular ; simple in the extreme, yet displaying the utmost fertility.

The theory which has served to develop these laws, rests entirely upon a fact the existence of which had been hitherto rather presumed than demonstrated. It consists in this, that these small solids, which are the elements of crystals, and which I call their *integrant molecules*, have, in all those which belong to one and the same species of mineral, one invariable form, the faces of which are in the direction of the natural joinings indicated by the mechanical division of these crystals, and of which the respective angles and dimensions are given by calculation combined with observation. Besides, the integrant molecules relative to different species also have diversities among them more or less remarkable, except in a very few cases where their forms have characters of regularity, whence result, as it were, points of contact between certain species. It follows from this, that the determination of the integrant molecules should have a great influence over that of the species ; and this consideration has led me more than once, either to subdivide into several species a groupe which, in the ancient methods, form only one, or to refer and re-unite the scattered members of a single species, of which several distinct species had been made. Some of these separations and reunions, made at a time when analysis had not yet unveiled the true nature of the substances which were the object of it, are now confirmed by chemical results ; and I shall even venture to say, that upon the hypothesis that no mineral substance had been as yet decomposed, we might, by a continued investigation of the integrant molecules, form assortments, which we might be justified in regarding as belonging to so many species distinctly circumscribed*: so that, in order to distribute them afterwards in a well arranged method, it would be sufficient to have the analysis of one single body taken in each.

From this we may conceive in what sense to understand what I have above hinted at, namely, that to chemistry belongs the determination of species. It would perhaps be more correct to say that it completes this determination by making us acquainted with the principal molecules, of which the integrant molecules are the assemblages. Already it is easy to perceive (and the subsequent part of the work will contain several examples) how interesting it is that the inquiries relative to these two kinds of molecules should conspire towards one common object; that the chemist and the mineralogist should mutually enlighten each other by their labours; and that goniometry, which furnishes data for submitting crystalline forms to calculation, should be associated with the scales that weigh the products of analysis.

The principal object of this Treatise is to detail and develop a method founded upon certain principles, and which serves as a kind of survey to all the information presented by mineralogy, assisted by the different sciences which can go hand in hand with it in one and the same line. It is calculated to bring all the minerals known under one and the same point of view, in order to compare them with each other, to study their characters, and to investigate alternately by experiment and theory the different phænomena of which they are susceptible. Every thing which can procure the observer the double advantage of being at once guided and enlightened during his progress will be employed; and this upon the principle that every science embraces every other science necessary to its elucidation.

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* These assortments would not be limited to crystals properly so called: we might also include lamellated masses, or even those which cannot be subjected to a mechanical division: for these last have frequently, when compared with analogous crystallized substances, a relation, in point of position and aspect, which ascertains them to belong to the same species: and thus these masses, insignificant in themselves, may be determined, at least intermediately, by the assistance of crystals which serve them in some measure as interpreters.

Mineralogy, in order to be successfully cultivated, requires extensive preliminary knowledge and persevering industry. It is the lot of all the sciences, that, in proportion as they acquire new degrees of perfection, they require additional efforts also, in order to attain the point at which, as from an elevated and commanding eminence, we can embrace at one glance a greater number of truths.

The result of my investigation, even supposing it to be as complete as possible, could not be regarded but as an introduction to the study of Nature. The different substances of which the globe is composed, placed in their respective positions by the concurrence of various causes, the actions of which have been directed by the Supreme Being towards the object proposed by his wisdom, present a spectacle perfectly novel, even to the eye the most familiarized with the aspect of minerals transported from the bowels of the earth into our collections. Here we see them collected and disposed in a perfectly symmetrical order; and Nature, breaking through on all sides the artificial limits traced by our systems, separates what we had united, while she associates and confounds those which we had separated. On one hand she exhibits, by striking contrasts, substances which touch and adhere together; and on the other hand she exhibits certain gradual transitions from one substance to another:—those are the successions of shades, which call upon a judicious observer to remark: *Here, the substance before us is no longer such a mineral, nor is it any longer such another.*

We may easily conceive how useful and even necessary a preparatory study is to the naturalist, to enable him to derive more benefit from his travels, and from observations made upon the spot. Objects already familiar to him, dispose him to form an acquaintance with those which will be new to him: he has not yet seen Nature herself, but he has received eyes for the purpose.

Although the observations here alluded to belong to a branch of science which has been called *geology*, the knowledge to which they lead appertains too closely to mineralogy to be omitted in a treatise relative to this last science. I shall confine myself to the mention of some general facts, the existence of which is confessed by several celebrated geologists; and shall subjoin an abridged description of the various aggregations known by the name of *rocks*, and of others which are nothing else than groupes or mixtures of mineralogical species. Those who desire more detailed notions may derive them from the works of Deluc, Saussure, Dolomieu, Pallas, Ramond, and other scientific men who have seen Nature upon a large scale, and have acquired from her a right to describe her phænomena.

But independently of those who are led by a particular taste towards researches which are the result of travels and voyages, there exist men every where, who, while residing in towns, are desirous of procuring useful information respecting the various mineral productions of Nature; and mineralogy has this advantage over the animal and vegetable kingdoms, that the collections of objects connected with it are more plentiful, and susceptible of fewer chasms, on account of the smaller number of species, while they are also less exposed to deterioration, and may be studied with delight at all seasons and in all places. I have flattered myself that there would be found in this work an additional facility for acquiring the knowledge so proper for adorning reason and cultivating the mind, and for exciting in the soul a becoming gratitude for the benefits conferred by an all-seeing Providence. With the view of attaining every object connected with the science, I have given, as often as opportunity offered, an idea of the purposes to which the minerals are applied, and of the processes employed by artists in order to render them fit for the use of mankind.

To return to the method which I have adopted in the classification of minerals. In the first place I resolved to direct my steps, as far as I could, by chemical results. Where, in fact, can we find relations more proper for closely connecting various mineral substances with each other, than those which are founded upon the existence of one identical principle? Where can we find differences more striking, between the same substances, than those which depend upon principles peculiar to each? Now, when we classify the substances of one and the same kingdom, we establish a continued comparison between them, according to the relations which connect and the differences which separate them. This comparison will therefore be the most exact, and at the same time the most natural possible, and the least arbitrary, if the method chosen for establishing it is that which unveils to us the intimate composition and foundation of each substance, which teaches us what it is in itself, rather than that which only shows us the outlines, or perhaps the external effects.

We may remark, before going further, that there are in the present case two problems to solve. The first consists in dividing and subdividing the collection of substances which a system should embrace, so that each may hold its true place. This is called *classifying*. The second has for its object the furnishing of easy and convenient methods for characterizing each substance in such a manner that we may ascertain it, wherever it presents itself, and discover in the system the place which has been assigned to it. The solution of the first of these problems is the sole object at present.

Let us now examine what are the resources presented to us by the present state of science, in order to attain this object. Among the minerals which in the common methods compose the class of stones, there are several in which analysis has demonstrated the presence of an acid combined with an earth. Such are the calcareous carbonate of the modern chemists, calcareous fluate, barytic sulphate, &c. Other substances, such as the emerald, topaz, garnet, &c., have only presented earths combined with each other, and sometimes with an alkali. We shall for a moment lay aside these last substances, in order to speak of those which contain an acid in their composition.

Here an important consideration presents itself relative to the distribution of these compounds. The modern chemists, in forming the table of the results of that new system which changed the face of science, by arranging in genera and species the acid substances, made choice of the acids for characterizing the genera, and distinguished the species according to the diversity of the bases united in succession with one and the same acid. This method of classifying seemed to be pointed out by the course of their operations alone. Oxygen being the acidifying principle, the common generator of the acids would become, by this kind of universality, the primitive substance, the different combinations of which with the different acidifiable bases we should first consider: and by a natural consequence, the acids resulting from these combinations would become, in their turn, the general terms to which we should refer the classification of the different and more compound substances of which they form part. The activity and energy of those principles which have so strong a tendency to unite themselves with the earths, the alkalis, and the metallic oxides, and seemed to rule over the combinations into which they entered, presented a new reason for assigning them the first place in these very combinations in which they then formed the principal part. But the mineralogist, whose object simply is to apply the results of analysis to the works of Nature, sees things in another point of view, and is necessarily led to choose the most fixed principles, as the common ties of the different species which ought to concur to the formation of genera.

In order to place this truth in its proper light, we may remark that, among the metallic substances which form one of the great divisions of the mineral kingdom, several admit an acid into their composition: hence it results in the first place, that, by giving the first rank to the acids, we could not avoid associating together in one and the same genus, on the one hand, carbonate of lead with carbonate of lime and barytes; on the other hand, the sulphate of iron with the sulphate of lime and that of magnesia; and so on with several other relations, in order to preserve the unity of the genera. Besides, by reasoning from combustibles, which frequently form part of the acids, as with these acids themselves, we should be forced to place together the sulphuret of iron, the sulphuret of lead, the sulphuret of zinc, &c. This is not all: the oxygen which should have determined the preeminence granted to the acids of which it is the generator, would obtain it for itself for a stronger reason, relative to its combinations with the metals, known by the name of *metallic oxides*, which would still form a single genus. It would remain to mark these places of the native metals in this distribution, and it seems that the only part to take would be to associate them also in one and the same genus.

[To be continued.]

LXX. *A new Method for detecting Arsenic.* By JOSEPH HUME, *Esq., of Long Acre, London.*

To MR. TILLOCH,—SIR,

FEW chemical tests are so interesting as those which discover the presence of a poison, particularly that of *arsenic*. It is not merely to the chemist or the mineralogist that such assistance is advantageous, but it is often of the greatest importance to the administration of public justice, where the innocence or guilt of the accused depends frequently on no other evidence than the existence of this most deleterious substance.

The methods principally adopted are few, perhaps not more than five; and though either of these, in many instances, may sufficiently answer the end, yet, when the quantity of the arsenic is extremely minute, I fear these are liable to objections, and the results may be ambiguous.

The latest observations on this subject are, probably, those of doctor Bostock, which were read before the *Liverpool* Medical Society. As I have not seen that gentleman's paper, excepting merely so much as is detailed in the critical analysis of books, published in the last number of "Medical and Physical Journal," I am not aware of any new instructions or cautions to render the usual methods more certain; but the test which I propose as a substitute, appears to be more efficacious, in as much as it produces a more *copious* precipitate from a given quantity of the arsenic; the result in all cases must, therefore, be nearer the truth, being more evident to the senses.

One experiment will sufficiently elucidate the plan I pursue. Let one grain of white oxide of arsenic and the same quantity of carbonate of soda be dissolved by boiling, in 10 or 12 ounces of distilled water, which ought to be done in a glass vessel; to this let a small quantity of the nitrate of silver be added, and a bright *yellow* precipitate will instantly appear. This is a more decisive test than sulphate of copper, which forms Scheele's green (arseniate of copper); and though my process answers very well with potass or even lime-water, yet I am inclined to prefer the common subcarbonate of soda. I remain, sir, your obedient servant,

Jos. Hume.

Long-Acre,
May 19, 1809.

LXXI. *On the present Mode of finding the Rates of Timekeepers. By a Correspondent.*

Of all the requisites by which a seaman is enabled to conduct his ship from one distant climate to another, no one appears to be of greater importance than an accurate knowledge of the time. The parliament of Great Britain, aware of its necessity, have, for the last century, offered a large reward to any person who may contrive a machine, that will keep time within certain limits of error during a long voyage. So anxiously has this been desired by that part of the nation who have any interest in its commercial or maritime concerns, and so great has been the honour awaiting the person who shall produce this desideratum, joined to the incitement naturally arising from the hope of obtaining a large reward, that many ingenious watchmakers and mechanics, both at home and abroad, have exhausted their utmost skill in the endeavour to bring it to perfection. Although they have hitherto found this impracticable, yet by the repeated attempts, and successive improvements of various hands, such an approximation to the truth has been attained, as reflects great credit on their ingenuity; and it would be unjust not to allow them this tribute of praise, when, for want of reaching precisely to the point required, they are deprived of those advantages, and remunerations for their trouble, which would in that case so justly become their due. Too frequently indeed does it happen in this country, that the most useful and ingenious mechanics, who have been reduced to indigence by attending perhaps to contrivances for the general good, rather than to their interest by labouring in the old beaten track, are suffered to pine in want and languish in distress; whilst pretenders and quacks have risen to affluence, basking in the sunshine of favour, although deluding the public with one hand, and picking their pockets with the other.

Few persons who have read the marquis of Worcester's Century of Inventions, and know the fate of his machines, have not regretted, that no attention was paid to his petition, for pecuniary assistance to enable him to complete these inventions, and publish them for the general benefit of mankind; more especially, as we are now convinced from the circumstance of many of them having been reinvented, that they were not the idle fancies of a lively imagination, but that they were realities, which he had actually constructed and applied to practice. Indeed at present, there is not any one department of the abstruse sciences, which can boast of receiving that encouragement or support, which, from its value to a commercial nation like Great Britain, it has a right to expect.

It may appear rather extraordinary at this enlightened period, when so many improvements have been made in instruments, and so great a degree of accuracy attained in practical astronomy, that the present mode of ascertaining and applying the rate of a timekeeper, practised in our fixed observations, should be called in question: and although I have looked over most of the publications on this subject, yet I am not aware of any arguments by which it can be justified.

The object intended by obtaining a rate, is to predict how far from truth the chronometer will be at the end of a given time; and to ascertain the degree of dependence that can be placed upon it, during the intermediate part of that time: or, to find whether the quantity of error increases uniformly and regularly in proportion as the time increases. It would seem, that in all the trials hitherto made of timekeepers in fixed observations, this object has only been partially pursued; whilst another of equal importance has been altogether neglected: that is, no method has been adopted, for finding the unavoidable alteration in the rate, produced by the different changes of temperature, to which most long voyages are liable: but on the contrary, the rate has been ascertained, merely for that temperature which happened to occur whilst it continued under trial, without endeavouring to find, whether any change would take place in the rate; if a material alteration should be produced in the weather, from heat to cold, or from cold to heat.

Can it reasonably be expected, in machines like these, that a rate found in a temperature of 30° of Fahrenheit, without any greater variation than 10° on either side, will be adequate to compute forward, and find the error of a watch that is afterwards to be kept going in a temperature of 100° or higher, wherein the expansion of the metals is so different from that of the former? It has also frequently happened, that the rate has been obtained in the coldest part of winter, when metals are most contracted, and applied to the hottest part of summer when they are most dilated, without any correction for the unavoidable defect of exact compensation; or that the rate has been found in a very cold latitude, to apply to the going of the watch in a very warm one; and *vice versa*.

Perhaps it may be asked here, Then of what use is your compensation? To which I reply, The value of this compensation in the balance is not depreciated because the utmost degree of perfection cannot be attained in adjusting it, any more than the value of the chronometer because it does not keep exactly with mean time; or any more than the indirect method of finding the true from the mean anomaly, because it is done by means of an approximation.

When chronometers have been sent on trial for twelve months

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months or more, it has been the general practice to compute forward with the first month's rate, (no matter what time of the year, or in what temperature it has been taken,) and to compare this computed error with the actual error shown by the watch, at the end of each succeeding month that it continued under trial. Now it is evident, that unless the timekeeper could be accurately compensated for the effects of heat and cold, which is seldom the case, there must arise a very material difference between these two rates, when any change has occurred in the temperature; and that a very small defect in the compensation must produce a very large deviation from the computed rate, by placing the watch to go in a different temperature, whether considerably warmer or colder, than that in which the first month's rate was found. Hence, if it should so happen, that the month's rate on which this computed error is founded, has been taken in January, when the thermometer was at 30°; then in July, when it is at 80°, the error in some cases becomes immense, but in most cases of too great consequence to be altogether neglected. Indeed, let this be taken in any part of the year, there is a great probability against its having been taken in that month, wherein the mean temperature occurred, of that season during which the watch continued there for trial. On the contrary, if the change of rate arising from the alteration of temperature be taken into account, and applied with the computed rate, an essential defect in the going of watches will thereby be obviated, and they will be found to have gone considerably nearer than people now believe they have.

Some of the best makers of chronometers, at the time of delivering them to the purchasers, have told them, how much the daily rate would vary between the heat of summer and the cold of winter. Although this is but a vague sort of statement, yet I believe few seamen, who know how, have failed to take advantage of it, and to apply it on all occasions where they could. But it has met with a very different fate in our observatories, and has not only not been applied, but has been condemned, and declared improper to be admitted there. Indeed, we have one publication extant, wherein it is expressly asserted, that as the act of

parliament

parliament makes no mention of *the rate,* therefore the allowance of a rate is an *indulgence;* and that the commissioners might require the maker to adjust the watch, so as to keep mean time accurately. With as great propriety might it be said, that the placing a glass over the face to view the hands and figures is an *indulgence;* or that to make the balance of two or more metals is an indulgence; or that the watchmaker having the power of choosing his own escapement is an indulgence; or lastly, that the astronomer being allowed to take the moon's place from the Nautical Almanack, instead of computing it with proper data and La Place's equations, is an indulgence. The act prescribes no definite means by which the object is to be attained, but leaves the artist entirely to his own choice: it cannor therefore but appear extraordinary, that the act should be thus construed, to the exclusion of the most essential part of the principle on which the method is founded.

Those who are acquainted with the adjustment of the balances of timekeepers know, that it is almost an impossibility to bring them precisely to that minute point of exactness, by which alone they keep accurately with mean time: and the difficulty of adjusting the balances for the effects of heat and cold, so that they shall never vary with the greatest extremes of either, would be at least as difficult to accomplish. But the former is attended with no other trouble to the practical navigator, than merely requiring the aid of a little calculation to allow for the deviation. It would be precisely the same with the allowance for the effect of the alteration of temperature: and it cannot therefore but appear extraordinary, that any objection should be made against applying this correction, when, by means of it, so much greater dependence can be placed on the time shown by the machine. Art has always lent her friendly aid to science, and science should return the kindness. Little can be expected in the progress of the longitude by either of them separately; but when they cordially unite their efforts, what is there that they cannot subdue?

Nothing would tend more powerfully to advance the interest of our own countrymen, than the establishment of a public

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public observatory, for trying timekeepers and keeping their rates; to which every maker of them, if he thought proper, might have access at stated hours, and be allowed always to keep there a certain limited number of pieces. Here he could try the effect of improvements, and gain experience thereby; then alter, and try again, until he succeeded to his mind: an advantage which he could not perhaps enjoy in his own house, for want of instruments of sufficient accuracy, and leisure to make the necessary computations.

A book containing the rate of each timekeeper belonging to each person might be kept, always ready for the use of the owner, and, if he thought proper, for the inspection of the public at large; by which, he would be enabled to fix a price on the machine, proportioned to the excellence of its going, and avoid all suspicion of partiality in giving the rate of his piece to the purchaser.

From this place, captains of ships or others might always be furnished with timekeepers, suitable to the price they could afford, or adapted, with respect to accuracy of going, for the purpose they might be required to execute.

In short, so many advantages would evidently be derived to the makers, and the public, by an institution of this kind, which could not fail to bring forward deserving merit as a claimant on public favour, that I am surprised the watchmakers have not established one at their own expense, by subscription, as the amount of it when divided between a number of proprietors would not be an object to each individual.

There are many situations near London that are well adapted for the purpose; the instruments necessary for it would not be expensive; and a steady careful person, capable by his scientific knowledge of conducting it with ability, might no doubt be found, who, considering it as an amusement rather than a labour, would be moderate in his terms for the discharge of a duty, which must evidently be beneficial both to the venders and the purchasers of these useful and necessary machines.

T.

LXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

MAY 4.—A paper was read on the triple sulphuret of lead, copper, and antimony, discovered by count Bournon in Cornwall. Mr. Jameson proposed to call this mineral Bournonite; but the count, in this additional memoir, in which he corrects the mistakes he made respecting the figure of its crystals in a former paper, prefers the name of the place where it was found. The integral molecule of this mineral he has determined not to be a perfect cube, as at first concluded, but having dissimilar sides in the proportion of 3 to 5. The count, in answer to a paper of Mr. Smithson in the Philosophical Transactions, defends the existence, not only of binary but also of ternary and quaternary compounds, and proves that the mineral in question is an example of the latter combination, a quaternary sulphuret. As an instance of the very singular difference in minerals in consequence of this variety of combination, the count refers to the anhydrous sulphat of lime, which is so very different from common gypsum, although composed of the same materials with the addition only of a little water.

May 10.—A paper by Mr. Home, on the *Squalus maximus*, was read, stating some particulars of the dimensions and conformation of the different basking sharks which have been thrown on the coast of Britain in the course of the last year. The author considers this species of shark as occupying an intermediate place between the mammalia (whales) and fishes, and partaking of the characters of both.

May 18.—Capt. Burney stated to the Society some more particulars respecting the floating of heavy bodies in a stream, and the nature of their moving faster than the current. He seemed to consider the cause of all such motion to be owing to the pressure of the atmosphere.

Mr. Cavendish laid a paper before the Society on the methods of dividing mathematical instruments, in which he proposed to substitute a balance compass and microscope for Mr. Troughton's cylindrical ruler. The plan was illustrated by a drawing of the instrument, which effected the purpose without the necessity or risk of calculations, which almost always involve errors.

A part of a letter from Dr. Henry to Mr. Davy, on oxygen of ammonia when exposed to electrization, was read. By some recent experiments Dr. H. has ascertained, that in decomposing the ammonia, some oxygen was admitted in the process, and that consequently what was found as the result of his former experiments was not derived from the ammonia by electrization, but from the agents employed. The final result, however, of his experiments proved that ammonia, as Mr. Davy originally concluded, is composed of oxygen, hydrogen, and nitrogen.

The Society then adjourned over one Thursday on account of the holidays.

SOCIETY OF ARTS, ADELPHI.

At a late meeting of this Society, a communication from Mr. R. Porrett jun. was read, announcing that he succeeded in obtaining prussous (or sub-prussic) acid—an acid differing from prussic, as sulphurous does from sulphuric acid, by containing less oxygen. It is a most delicate test of the presence of silver in solution, and has the singular property of precipitating iron of a red colour. It has completely proved the presence of oxygen in prussic acid; as by deoxygenating the latter it becomes prussous acid; and on adding oxygen, it is again capable of affording a blue precipitate of iron.

A new process for hardening the surface of casts in plaister of Paris, has been communicated to the same Society. It consists in boiling the cast in a solution of one pound of alum in a pint of water for 15 minutes, and then suffering it to dry gradually for about a month; in this way the cast acquires a very considerable degree of hardness upon its surface, and is even capable of receiving a polish by friction, so as to resemble white marble; and the surface of it may be cleaned from time to time, without the least injury to the sharpness of the cast.

WERNERIAN NATURAL HISTORY SOCIETY.

At the meeting of this Society on the 8th of April, there was read the first part of a Description of the Mineral Strata of Clackmananshire, from the bed of the river Forth to the base of the Ochils, illustrated by a voluminous and very distinct plan or section of those strata, done from actual survey, and from the register of the borings and workings for coal in Mr. Erskine of Mar's estate in that district; communicated by Mr. Robert Bald, civil engineer, Alloa. In this first part Mr. Bald treated only of the alluvial strata. In continuing the subject, he is to illustrate it still further by exhibiting specimens of the rocks themselves.

Mr. Charles Stewart laid before the Society a list of insects found by him in the neighbourhood of Edinburgh, with introductory remarks on the study of entomology. It would appear that the neighbourhood of Edinburgh affords no very peculiar insects, and but few rare ones. The list contained about 400 species; which, Mr. Stewart stated, must be considered as the most common, as they were collected in the course of two seasons only, and without very favourable opportunities. It was produced (he added) merely as an incitement to younger and more zealous entomologists.

At this meeting there were laid on the Society's table the first two volumes 4to, with a volume of figures, of Comte de Bournon's System of Mineralogy; presented by the author.

At a meeting of this Society on the 13th of May, the second part of Mr. Bald's interesting mineralogical description of Clackmananshire was read, giving a particular account of two very remarkable *slips* or *shifts* in the strata, near 1000 feet in depth, and by means of which the main coal field of the country is divided into three fields, on all of which extensive collieries have been erected.

The Rev. Mr. Fleming, of Bressay, laid before the Society an outline of the Flora of Linlithgowshire, specifying only such plants as are omitted by Mr. Lightfoot, or are marked as uncommon by Dr. Smith. This, he stated, was to be considered as the first of a series of communications illustrative of the natural history of his native country.

Mr. P. Walker stated a curious fact in the history of the common eel. A number of eels, old and young, were found in a subterranean pool at the bottom of an old quarry, which had been filled up, and its surface ploughed and cropped, for above a dozen of years past.

The secretary read a letter from the Rev. Mr. Maclean, of Small Isles, mentioning the appearance of a vast Sea Snake, between 70 and 80 feet long, among the Hebrides, in June 1808.

And he produced a list of about 100 herbaceous plants, and 200 cryptogamia, found in the King's Park, Edinburgh, and not enumerated in Mr. Yalden's catalogue of plants growing there; communicated by Mr. G. Don, of Forfar, late superintendant of the Royal Botanic Garden at Edinburgh.

MANCHESTER PHILOSOPHICAL SOCIETY.

We copy from a Manchester newspaper the following Resolutions, which were occasioned (as appears from the advertisement containing them) by some extraordinary circumstances, that have lately occurred in the Literary and Philosophical Society of that place. They were passed unanimously at a meeting of the Society, which was held on the 5th instant, in consequence of a special requisition; and which was attended by a greater number of members than had ever been assembled on any former occasion: consistently with the spirit of them, Mr. Henry was re-instated in the office of president on the 12th instant.—

"At an extraordinary meeting of the Literary and Philosophical Society of Manchester, held on Friday, May 5, 1809, in consequence of a special requisition signed by twenty-nine members, it was resolved unanimously,

"1st. That the thanks of this meeting are due to Mr. Henry, for his long and valuable services, and for his uniform exertions to promote the best interests of the institution,

"2dly. That the circumstances of Mr. Henry's age, and standing in the Society, the great respectability of his character, his valuable contributions to the Society's Memoirs, and the rank which he has long held in the scientific world, peculiarly distinguish him as a fit person to fill the chair of this Society.

"3dly. That a deputation be appointed to wait upon Mr.

Henry, to communicate, in the most respectful manner, the sentiments expressed in the two foregoing resolutions."

STONYHURST SCIENTIFIC ESTABLISHMENT.

When the French entered Liege, the gentlemen of the seminary at that place were forced to make a precipitate retreat, abandoning a large establishment, together with a valuable library and a fine collection of mathematical instruments. Having since found an asylum in this country, they have formed an establishment at Stonyhurst, where they are making a laudable attempt to introduce the sciences, in their improved state, into their common course of education. As a first step, a handsome room for a library and another for mathematical apparatus have been built, to which it is intended to add a chemical laboratory as soon as possible.

As the arrangements of the building appear to us to unite much in a small space, we are happy in being enabled to present our readers with an engraving of the ground plan, (see Plate XII.) which may prove useful to those who project similar establishments.

It is not doubted that the gentlemen at Stonyhurst will not only be soon enabled to finish the erection of their building, but to procure the books and instruments necessary to the perfecting of their undertaking—a very liberal subscription having been procured among the friends to their establishment.

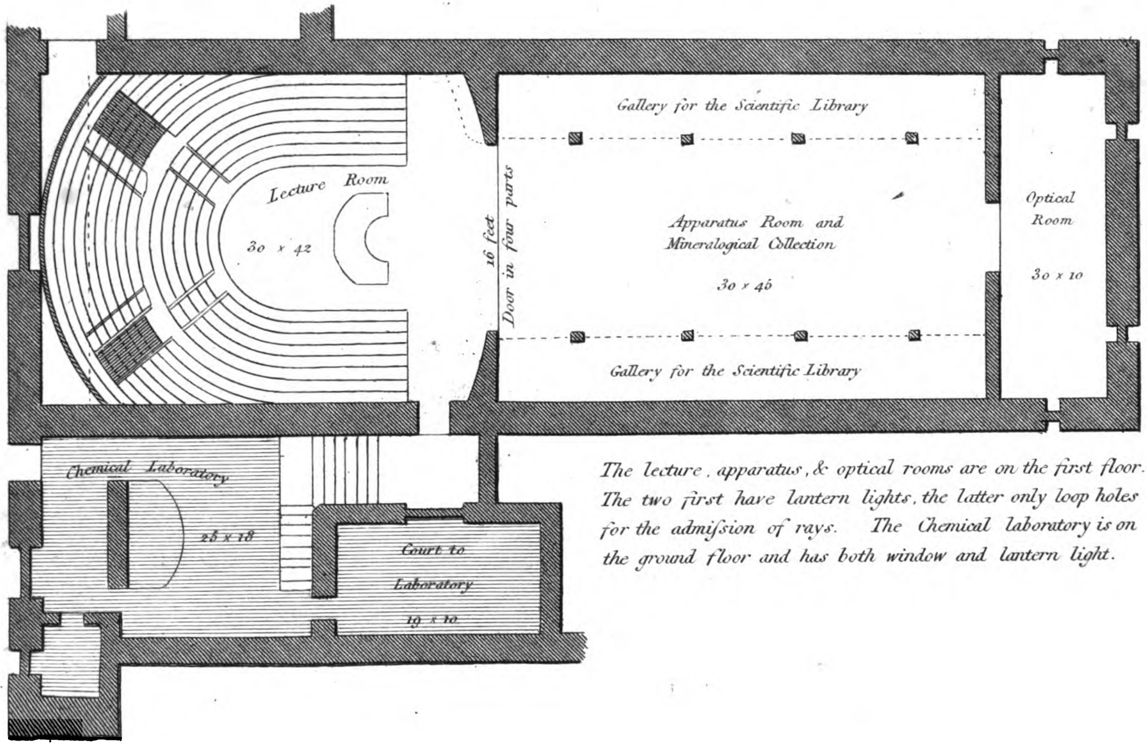
Among other respectable names in the list of contributors we observe the duke of Northumberland's for 100*l.*: the marquis of Buckingham subscribes 50*l.*, the earl of St. Vincent 50*l.*, and the earl of Moira 50*l.*, &c. &c.

LXXIII. *Intelligence and Miscellaneous Articles.*

SMYRNA MADDER.

This valuable plant has lately been introduced into this country by Mr. Spencer Smith, who furnished the Society of Arts, &c. &c., with some seed, from which Mr. Salisbury of the Botanic Garden, Cadogan Place, Sloane Street, has been so fortunate as to obtain plants, which have grown in

GROUND PLAN OF THE STONYHURST SCIENTIFIC ESTABLISHMENT.



The lecture, apparatus, & optical rooms are on the first floor. The two first have lantern lights, the latter only loop holes for the admiſsion of rays. The Chemical laboratory is on the ground floor and has both window and lantern light.

W. Skelton sculp.

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in a most promising manner. He expects to obtain seed from them, and by their cultivation hopes may now be entertained that this most valuable dye-root will become naturalized to our soil.

LECTURES.

Mr. Brookes's Summer Course of Lectures on Anatomy, Physiology, and Surgery, will commence on Saturday the 10th of June, 1809, at Seven o'Clock in the Morning, at the Theatre of Anatomy, Blenheim-Street, Great Marlborough-Street.

Surgeons in the Army and Navy may be assisted in renewing their Anatomical Knowledge, and every possible Attention will be paid to their Accommodation as well as Instruction.

Anatomical Converzationes will be held weekly, when the different Subjects treated of will be discussed familiarly, and the Students' views forwarded—To these none but Pupils can be admitted.

Spacious Apartments, thoroughly ventilated, and replete with every Convenience, are open at Five o'Clock in the Morning, for the purposes of Dissecting and Injecting, where Mr. Brookes attends to direct the Students, and demonstrate the various Parts as they appear on Dissection.

An extensive Museum, containing Preparations illustrative of every Part of the Human Body, and its Diseases, appertains to this Theatre, to which Students will have occasional Admittance—Gentlemen inclined to support this School by contributing preternatural or morbid Parts, Subjects in Natural History, &c., (individually of little value to the Possessors) may have the pleasure of seeing them preserved, arranged, and registered, with the Names of the Donors.

TERMS.	£.	s.	d.
For a Course of Lectures, including the Dissections,	5	5	0
For a Perpetual Pupil to the Lectures and Dissections,	10	10	0

The Inconveniences usually attending Anatomical Investigations are counteracted by an antiseptic Process. Pupils may be accommodated in the House. Gentlemen established in Practice, desirous of renewing their Anatomical Knowledge, may be accommodated with an Apartment to Dissect in privately.

Mr. Taunton will commence his Summer Course of Lectures on Anatomy, Physiology, Pathology, and Surgery, on Saturday the 3d of June, 1809, at Eight o'Clock in the Evening precisely. They will be continued every Tuesday, Thursday, and Saturday, at the same hour. Particulars may be had on applying to Mr. Taunton, Greville-Street, Hatton-Garden.

LIST OF PATENTS FOR NEW INVENTIONS.

To Simeon Thompson, of Maddox Street, Hanover Square, for a machine or machinery for raising, lowering, drawing, driving, forcing, impressing, or moving bodies, substances, materials, fluids, articles, or commodities.—March 20, 1809.

To Charles Valentine, of the parish of St. James, Clerkenwell, japanner, for a new mode of ornamenting and painting all kinds of japanned and varnished wares of metal, wood, paper, or any other composition, and various other articles.—March 20.

To James Yonnie, of Theobald's Row, Middlesex, smith, for a machine or instrument, to be applied to stoves or grates, for preventing accidents by fire; and whereby the fires in stoves or grates may be put out and extinguished with safety and facility.—March 28.

To Elizabeth Perryman, of Greek Street, Soho, Middlesex, for a new street and hall lamp, and the necessary apparatus for expediting the trimming, lighting, and cleansing the said street and hall lamp.—March 29.

To Richard Willcox, of the parish of St. Mary, Lambeth, Surrey, mechanist, for sundry apparatus or machinery for accelerating the manufacturing of felt or stuff hats; and for cutting and removing by machinery the furs of beavers, rabbits, and the whole variety of skins, the furs or wool of which are used for the purpose of hat-making.—April 3.

To Richard Willcox, of the parish of St. Mary, Lambeth, Surrey, for certain machinery for facilitating the manufacturing of stuff, wool, and other hats, and bonnets felted.—April 3.

To John Thomas Groves, of Great Scotland Yard, Whitehall,

hall, Middlesex, esq., for an improved mode of constructing buildings, by which great expense, labour, and time, is saved, and the buildings secured from dry rot, with other advantages.—April 3.

To John Frederick Archbold, of Great Charlotte Street, Surrey, gent., for a method of converting salt or sea water into fresh water, both on land and on board of ship at sea.—April 18.

To William Pleasants the elder, of Abbey Street, in the city of Dublin, bachelor of arts, for a self-mover, or machine which can keep itself in motion.—April 19.

To Phillips London the elder, and Phillips London the younger, of the parish of St. Luke, Chelsea, Middlesex, gents., for certain new and improved methods or processes of manufacturing, refining, and purifying muriate of soda or common salt.—April 19.

To Phillis Bown Thomason, wife of Edward Thomason, of Birmingham, manufacturer, for improvements in the making of umbrellas and parasols.—April 19.

To Matthias Wilks, of Brabant Court, in the city of London, merchant, for his compound substance or cake for the feeding of horses and other animals.—April 20.

To John Barton, of the town of Tunbridge, in the county of Kent, gent., for his machine for raising weights or water with greater facility and at less expense than any at present used.—April 25.

To Richard Trevithick, of Rotherhithe, in the county of Surrey, engineer, and Robert Dickinson, of Great Queen-Street, in the county of Middlesex, esq., for certain inventions calculated to improve naval architecture and navigation, and to contribute to the comfort and better subsistence of mariners.—April 29.

To William Hamilton, of Lower Mount-Street, in the city of Dublin, for his new mode of preparing soda and other mineral waters, spirituous, acetous, saccharine, aromatic liquors, and sundry improvements relative thereto.—May 4.

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METEOROLOGICAL TABLE,

By Mr. Carey, of the Strand,

For May 1809.

Days of the Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.			
April 27	49°	52°	47°	29·60	39	Cloudy
28	48	55	40	·46	10	Rain
29	40	45	38	·82	51	Cloudy
30	42	52	48	·69	55	Fair
May 1	47	52	44	·27	10	Rain
2	42	51	40	·53	14	Showery with thunder
3	40	52	42	·80	60	Fair
4	45	56	49	·90	10	Cloudy
5	49	55	43	30·02	80	Fair
6	45	59	49	·28	65	Fair
7	51	65	57	·34	65	Fair
8	57	65	51	·29	75	Fair
9	54	66	51	·14	76	Fair
10	53	67	54	·05	81	Fair
11	57	62	57	29·99	95	Fair
12	57	72	57	30·00	82	Fair
13	58	73	60	29·91	79	Fair
14	61	74	62	·89	70	Fair
15	61	64	56	·80	40	Showery
16	60	73	61	·78	78	Fair
17	63	73	62	·85	80	Fair
18	66	75	63	·75	75	Fair
19	67	75	55	·52	46	Showery with thunder
20	58	65	54	·78	36	Cloudy
21	55	59	51	·90	39	Cloudy
22	53	66	56	30·13	67	Fair
23	55	75	51	·17	63	Fair
24	51	67	50	·16	75	Fair
25	50	61	52	·02	44	Cloudy
26	51	66	55	29·82	34	Cloudy

N. B. The Barometer's height is taken at one o'clock.

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LXXIV. *Thoughts on Atmospheric Density and Pressure.* *By* THOMAS CHARLTON SPEER, *Esq.*

To MR. TILLOCH,—SIR,

NOTWITHSTANDING the wide field of inquiry which the mechanical history of the atmosphere presents to us, and notwithstanding the interest which such inquiry must naturally excite in a philosophic mind, yet we must confess, our labours in it have been but partial, and our knowledge of it is but limited:—the attention of men of science has (comparatively speaking) been mostly confined to its chemical history, probably from its more immediate connection with, and relation to, practical and useful results. On the former subject, therefore, I beg to offer some ideas which have suggested themselves to me, and which relate to one of its principal (though I think least understood) properties.

Unaided by the lights of natural philosophy or the force of experiment, it scarcely comes within the limits of human conception, that that invisible inodorous aëriform mass of fluid surrounding our globe should at all be subject to the laws or possess the properties of matter; but particularly that it should possess either *density*, *weight*, or *pressure*.

These three properties of atmospheric air, viz., *density*, *weight*, and *pressure*, are often misunderstood, and generally confounded with each other, particularly the two former. Now we well know that attraction of cohesion and attraction of gravitation sensibly differ from each other; if not, platina would be the hardest, and the diamond the heaviest, bodies in nature. We know that their action is quite different, that of the one being inversely as the squares of the distances, that of the other increasing at a much quicker rate as bodies approach, the smaller the distance the greater its power. By the word *density* therefore, (which in particular seems often misapplied and misunderstood,) I mean, strictly speaking, *impermeability*, or that power in a body by which it is enabled to resist or obstruct (more or less) the passage of other bodies through it, and which may be estimated by the greater or less difficulty with which such resistance or obstruction is conquered. Now this *impermeability*

ability in a body must arise not only from the closeness with which its integrant parts are connected, but also from the weight of these parts, and consequently the difficulty of removing them; so that, in other words, density, I think, may be defined, that power *resulting from the union of the attraction of cohesion and gravitation.*

That the atmosphere is possessed of this power is well known:—take two bodies of different weights but equal bulks, drop them from the same height, it will be found that the differences of their velocity in descent will be directly as the differences of their weights, and as the differences of the times of their descent, and that consequently the spaces of atmosphere descended are directly as the squares of the times, and as the squares of their velocity in falling. To this it may be answered, that gravity is the sole cause, and that the air has no effect whatever: however, that this is not the case is well known by the old experiment of a feather and guinea falling alike in the exhausted receiver of an air-pump. Hence, were it not for the greater or less resistance or impermeability of the atmosphere, the force of gravitation would be equal in bodies; their absolute gravity depends, more or less, on the density of the atmosphere, not the latter on the former.

Now it is obvious that this power can only exist where there are particles or molecules (see Boscovich): it must depend on the disposition of those particles, and even on the particles of those particles to the ultimate one, viz., on their size, their shape, form, &c., and their degree of aggregation and consequent distance from each other. Atmospheric air, therefore, must have its particles or molecules, (though completely imperceptible to our organs of sensation,) certainly, from its great permeability, its yielding to the slightest impression, and affording little or no resistance to the insinuation of bodies denser between the interstices of its particles: this power in atmospheric air may almost be said to be at its minimum.

This permeability in atmospheric air arises, it is supposed, from the very slight aggregation of its particles, and consequently their small quantity and great distance from each other:—

other :—however, this may not be the case; it (the atmosphere) may contain many particles, and these particles closely approximated together by a strong cohesive force; but this *permeability* may arise from their extreme minuteness and want of gravity, from their spherical form, (their angles being blunted off by friction,) and their thus sliding through, among, and under each other, yielding to, and being impelled by every motion communicated. Indeed the latter hypothesis, I think, seems the more probable. However, from whatever cause it is, a degree of tenuity or *permeability* arises which scarcely any other body possesses, and which may be estimated by the velocity with which heat, light and sound travel through it.

Atmospheric density is generally confounded with *atmospheric pressure*, though distinct from each other; the one is a property it possesses in common with all other bodies, the other is peculiar to itself alone. In the one there appears an inherent and self-existent direction of the particles, and this direction seems positive and determinate; in the other there appears no direction but what is given by external and accidental causes, and therefore quite vague and indeterminate, and only what is possessed by all matter. Atmospheric pressure may be shown in various ways, perhaps one of the simplest is thus :—invert over a bason of water, a tumbler previously exhausted of its air; the water of the bason will ascend in the tumbler much higher (according to the dimensions of its column) than its level in the bason,—being pressed down by the external atmosphere, its particles are forced to cohere closer together, until the force of pressure is withdrawn; they then recede from each other, the interstices between them are increased, and thus taking up a greater space, they consequently ascend, being prevented by the sides of the tumbler from expanding laterally.

Now surely it is inconceivable that a body weighing only the eight or nine hundredth part that of another, could possess the power of raising it up, and consequently forcing it to assume a contrary direction to that gravitating one which it, though in so slight a degree, possesses in common with other bodies, were it not for a self-impelling self-existing force in the atmosphere. For instance, were the experiment tried under a fluid still heavier than air, but lighter than water (oil for example), in a vacuum, *Quere,* Will not the water remain level inside and outside?

Hence we must argue, that pressure is distinct from density, self-existing in the atmosphere, and in that alone; nor even do they always act directly of each other, particularly in the upper regions of the atmosphere. (See Bouguer's Travels &c. in the Andes.) However, although they do not appear to be one and the same power, yet they seem intimately connected, and to act directly with each other, and what affects the one affects the other.

Galilei was the first discoverer of atmospheric pressure, by observing that fluids rose to a certain height in a vacuum, which had formerly been accounted for by the old idea of "Nature abhorring a vacuum."

Torricelli, his pupil, completed the discovery*, observing that the atmosphere pressed equally on all bodies at the earth's surface, and in proportion to their densities; viz., more on a fluid than a solid, and more on a gas than either: and as fluids are of different densities, and the only bodies susceptible of being acted on by this pressure, from the slight cohesion and consequent motion of their parts, he naturally conceived that the rarer the fluid the higher it would rise in a vacuum, and *vice versa.*

Thus, he first found that water, at the medium temperature of the atmosphere, would ascend about 32 feet, considering this the rarest fluid, which height or column of 32 feet was consequently an equipoise to a column as high as the atmosphere:—calculating the comparative densities of fluids, he conceived that one twice as dense as water would ascend half as high, and so on, the density of the fluid being inversely as the height of its ascent. Then, considering the comparative density of water, the rarest, with that of mercury, the densest fluid known, he found it to be 14·1; consequently he concluded that the mercury would rise $\frac{1}{14}$th the height of the water: this he found exactly to answer. He filled a glass tube about three feet long, closed at one end and exhausted of its air, and inverted it into a bason of mercury; the mercury rose 29 inches, = $\frac{1}{14}$th the height of the column of water; the remaining part of the tube was of course a vacuum (the Torricellian vacuum).

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* After Torricelli, Pascal brought this discovery to still further perfection, and made many important additions, particularly by his celebrated experiments on the Puy de Dome.

This mercurial column 29 inches high, is therefore equal to a column of the atmosphere extending from its summit to the earth.

The atmosphere presses equally on all bodies at the surface of the earth, or equally distant from it.

This pressure decreases in a direct ratio (generally) with its distance upwards from the earth's surface, (the atmospheric column being shortened,) or rather from the level of the sea, it being the only natural uniform level from whence such indications are deducible. Hence it follows, that it increases directly with its distance downwards from such level—(the atmospheric column being lengthened)—so that in pits the pressure must (comparatively speaking) be at its maximum: of this, however, I believe we have no direct proofs.

Thus the atmosphere may be said to constitute a mass consisting of strata lying on each other, each stratum pressing on the one next it with not only its own *individual* weight, but with that *collected* weight with which it is pressed from above, so that the lower stratum has the whole superjacent pressure as it were on its back, and with it presses the earth.

Hence (supposing the ratio to be direct), if heights in the atmosphere be taken in arithmetical proportion, its rarity (from want of pressure) will be in geometrical progression: thus, at 7 miles above the earth it is 4 times rarer, at 14 miles it is 16 times rarer, at 21 miles it is 64 times rarer, and so on, the rarity increasing in proportion to the height as 4·1.

This principle of the atmosphere decreasing in pressure as we ascend, and this decrease being (generally) equable, has been most happily applied in the estimation of altitudes (particularly those of mountains) by the Torricellian tube, or, as it is more commonly called, the barometer *. The mercurial column decreasing as we ascend, the column of air decreasing in height, and of course decreasing in its ability to support the former; at that height therefore at which the mercury would no more rise but remain in its bed, the pressure must either be at its limits, or too slight to raise so dense a body as mercury: but if a rarer fluid were employed at this height, the force of pressure might still perhaps sensibly act on it until that period, when, from the want of both density and pressure in the air, its particles could no longer keep together, and a vacuum must ensue: indeed this I think (if possible) the only true mode of ascertaining the height of the atmosphere, because *where* there is atmosphere *there* will be pressure also.

However, the barometer does not appear calculated for estimating great heights in the atmosphere, because *there*, as was said before, its decrease of density does not often keep pace with its decrease of pressure or gravity, as appears from the observations of Bouguer and Don Ulloa, &c. on the Andes, the upper regions being (from various causes) not subject to the same laws as the lower ones. Hence, as in the common indications of a barometer, the pressure, weight, and density are more or less connected with each other, though in reality, and strictly speaking, distinct, as before mentioned: consequently the equability of its ascension must, at these high regions, be considerably diminished, from these three powers being more or less at variance with each other, and therefore its indications must be erroneous. If the ratio of the mercury's descent for our ascent were known, and was equable, the barometer might ascertain any height, and we might easily know that at which it would remain at its level.

In the lower regions (the heights of mountains for instance) the mercury, it is said, generally falls an inch for 850 or 900 feet at a medium*: if this ratio held good altogether, the mercury must remain in its bed at the height of 27000 feet.

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* Barometer, however, in strictness, means a measurer of the weight of the air.

As a square inch column of mercury 30 inches high (which various observations and experiments have proved to be the medium pressure in those countries, or others situated in or near their latitudes,) weighs about 15lbs., so the atmosphere must press with a weight equal to 15lbs. on every square inch, or 180lbs. on each square foot: calculating thus, the whole superincumbent pressure of the atmosphere amounts to 12,043,448,800,000,000,000lbs.† Reckoning the surface of a man's body to be about 14 square feet, he sustains a pressure (calculating thus) of 11 tons 2 hundred weight 18½lbs.

It may be wondered how man could bear such a weight on his body and not be crushed. On the contrary, this pressure is indispensably necessary to our existence, and two reasons concur in preventing its being felt troublesome: 1st, its being so equal all over our bodies as not to move their fibres; 2dly, the caloric generally evolving from hence counteracts and renders it less sensible. Besides, sensations we have been accustomed to from our birth do not much annoy us, and it is probably this pressure that occasions the cries of the new-born babe.

Atmospheric pressure is necessary to our existence. In ascending into the air, we might suppose our sensations would be more agreeable by being loosed as it were from a heavy load: yet the contrary is the fact; the blood of our internal vessels not being pressed down, bursts and overflows its barriers, insomuch as sometimes to endanger death.

These sensations are always perceived in ascending considerable heights: spitting of blood, and bleeding at the nose and eyes, a drowsiness, listless apathy, and inexertion are experienced*, Hence Providence has wisely ordered it, that this pressure, though apparently such a load on us, should be absolutely necessary to our lives, by keeping our blood within its proper limits, making its particles retain their proper cohesion, and thus giving the pulse, heart, &c., that uniform gradation of beating in which the vital principle consists.

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* This appears from the observations and experiments of Saussure and De Luc on the Alps, Don Ulloa, Bouguer and Condamine on the Andes, and of Mr. Kirwan on mountains in Ireland; all of whose labours in this interesting subject, besides those of Dr. Halley, Sir George Shuckburgh, and General Roy, have so much contributed to its advancement.

† Equal to a ball of lead 60 miles in diameter. See Coles's Lectures.

Atmospheric pressure varies considerably, (as was before observed,) particularly in the upper regions, and these variations appear to increase as we ascend into them; because the decrease of density not keeping pace with the decrease of pressure, there must, in those high regions, be sudden condensations and rarefactions, which must divert from their direction the pressing down particles of the atmosphere, and drive them laterally and otherwise, &c. Hence the secondary cause that affects these variations is the winds: on this account these variations are greatest at the poles, and decrease towards the equator, there being greater winds at the former, the atmosphere there being denser from its less high state: that they are little or none at the equator, has often been proved.

Were it not for atmospheric pressure there would be but little distinction between liquids and gases, no more than what is produced by the action of that aggregate attraction on the one hand, and that quantity of caloric on the other, with which they are naturally endued. *For*, as the pressure always assists cohesion, and consequently counteracts the effect of caloric; and as the particles of bodies have liberty to move and recede from each other *directly* as the positive and repelling power, and *inversely* as the negative and approximating one, viz., cohesion; and as between these constituent internal powers all bodies are balanced; it follows, that when a third neutral external power is introduced, viz., pressure, it must turn the scale, weaken the action of the caloric, and tend to keep the body in a liquid state according to its force. Hence the quantity or degree of heat

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* Messrs. Humboldt and Bonpland perceived these symptoms to a very alarming degree on the Cordilleras: even Dr. Pitcairn in ascending Arthur's Seat (only 900 feet) began to be sensibly affected so.

necessary

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necessary to produce the conversion of a liquid into a gas (or evaporation) must depend on the pressure of the air: the greater the pressure, the greater the heat necessary from its greater resistance to its escape, and *vice versa.*

Thus, in the application of a heat (nearly boiling) to a fluid, its particles being rendered so rare and minute acquire a disposition to ascend, from want of gravity. In this disposition to ascend they come into contact with the particles of the air pressing downwards, and are thrown down again, their force of ascent being inferior to the force of descent possessed by the atmospheric particles. The body is therefore still kept liquid in this degree of heat: if, however, it is increased a little further, the particles are forced to rise up and conquer the force of pressure, and thus evaporation takes place.

In this state then a body is balanced between three powers, two of which may be said to be *quiescent*, and the third *divellent;* if the united sum of the quiescent forces, pressure and cohesion, amounts to more than the divellent caloric, its liquidity remains; if less, evaporation ensues.

Then, in a case of evaporation, to know the force or power with which it takes place, add the sums of the quiescent powers, cohesion and pressure, (numerically expressed,) deduct their united amount from the sum of the divellent caloric, and the remainder will be the sum required. Thus, suppose a body possessed with the three powers so, the quiescent A and B, and the divellent C, thus A with the force of 6, B with the force of 2, and C with the force of 10, then we have $A\,6 + B\,2 = 8 - C\,10 = 2$, the force with which the body evaporates. If the forces are equal, and that there is no remainder, the body of course will remain as it is.

On the other hand, in the reconversion of a gas into a liquid (or condensation) where the powers are reversed, to know the force with which the body condenses, subtract the quiescent caloric from the united sum of the divellents, pressure and cohesion, the remainder is the sum required. Thus, suppose a body with the powers to C, caloric with the force of 6, B pressure with the force of 8, and A cohesion with that of 2, then we have $C\,6 - B\,8 + A\,2 = 10 = 4$. The body therefore condenses with the force of 4. Thus then, by numerical expression, we find the conversion of liquids into gases, and *vice versa*; and hence this conversion depends more or less on atmospheric pressure.

Although the force of pressure is too slight to affect solids, yet crystallization is in a certain degree dependent on it; the cohesive power in the liquid would not be sufficient of itself.

Hence, though the points of condensation and evaporation are established at certain standards, it is merely because the pressure is similarly established at a certain standard point (30 inches). The boiling point of water at this pressure is 212°. On the tops of mountains a much less degree of heat will suffice*, and in an exhausted receiver it will boil at 70°.

On the contrary, when the pressure is increased (in pits or mines), a greater heat is of course required, and by artificial pressure water may almost sustain any heat without evaporating†. The exact ratio of the decrease of heat for the decrease of pressure, or the increase of heat for the increase of pressure, in the process of ebullition in water or other fluids, has not, I believe, been hitherto determined.

THOS. CHARLTON SPEER.

May 19, 1809.

LXXV. *On Geometrical Proportion.* *By* WM. MARRAT, *Esq., of Boston, Lincolnshire.*

To MR. TILLOCH,—SIR,

THE doctrine of proportion is well known to be of so much importance to mankind in general, that any attempt to elucidate its principles cannot be deemed entirely useless. A great part of the obscurity with which this subject is enveloped, arises from the vague and ambiguous manner in which authors have defined the word proportion: thus, it is frequently confounded with the words *ratio*, *reason*, *analogy*, &c., and sometimes *two*, sometimes *three*, and sometimes *four* quantities are said to be proportional. This mode of procedure creates a good deal of confusion, and not a little embarrasses the ideas of beginners; and it is to obviate, in some measure, these irregularities, that I now send you the following short disquisition on this interesting branch of science. I must observe further, that the manner which is generally practised by authors of treating the subject geometrically, as is done in Euclid's Elements, and most of the modern books of geometry, is certainly not the most eligible, or best adapted to learners; and, except the few trifling observations which may be met with in books of arithmetic, it is to books of geometry alone to which a learner can have recourse for any information he may require.

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* See Saussure, on the Alps, &c.

† Hence, by means of an instrument that would very sensibly measure the degrees of heat, we might (cæteris paribus) ascertain heights in the atmosphere, and our distance above it, by the *decrease* of the boiling point; and, on the other hand, our distance below it (in pits for instance) by its *increase*.

To prove that what I have advanced concerning the obscurity of the subject, when treated geometrically, is correct, I need only appeal to those gentlemen who are in the practice of teaching the fifth book of the Elements; it is well known that the difficulties attending it are so great, that very few students ever thoroughly understand his demonstrations, owing most probably to their not being able to form a correct idea of his criterion of proportion. Again, "Ordinary language (as Professor Playfair observes) conveys the ideas of the different operations supposed to be performed by these demonstrations so slowly, and breaks them down into so many parts, that they make not a sufficient impression on the understanding; and this generally happens when the things treated of are not represented to the senses by diagrams, as they cannot be when we reason concerning magnitudes in general, as in this part of the elements of geometry. It is obvious, therefore, that we ought to adopt the language of arithmetic, or algebra, which by its shortness, and the rapidity with which it places objects before us, makes up for its being a conventional language; and also for using symbols to denote the things we wish them to express."

The first ideas of proportion which we generally acquire, are obtained by comparing natural objects with one another:

thus, we say one thing is *twice*, *thrice*, &c., as large as another, or one thing will *cost* twice, thrice, &c., as much as another: but, as our ideas expand, we wish to compare *all kinds* of magnitudes as exactly as possible; and then it is that a more ample view of the subject becomes necessary. Arithmetic instructs us how to compare any *two* quantities with each other, so as to determine the relation which subsists between them: this is the first notion which we acquire of proportion, and it is the foundation upon which we must raise our future reasonings: this comparison of any *two* quantities may be called a *ratio*, and hence we have the following

Definition I.—The word RATIO signifies the relation which subsists between *two* quantities with respect to their magnitudes. One of the quantities thus compared is called the antecedent, the other the consequent of the ratio, and they are sometimes expressed by placing two points between them, or more frequently by writing them in the form of a fraction: thus, $3:4$, or $\frac{3}{4}$, is the manner in which we generally designate the ratio of 3 to 4, and $a:b$, or $\frac{a}{b}$, denotes the ratio of a to b.

The ratio of 6 to 12, or $\frac{6}{12}$, is the same as $\frac{3}{6}$, or as $\frac{1}{2}$; hence it is plain that the *terms* of a ratio may vary, and the ratio still continue the same: if, therefore, the terms of a ratio be either multiplied or divided by the same quantity, the ratio will not be altered; for $\frac{ma}{mb}=\frac{a}{b}$, and $\frac{\frac{a}{m}}{\frac{b}{m}}=\frac{a}{b}$ = the same ratio.

It will now be very easy to define the word *proportion.*

Definition II.—Four quantities are proportional when the *ratio* between the first and second is the same as the *ratio* between the third and fourth; and, in general, any number of quantities are in the same proportion when they are composed of equal ratios.

Thus four quantities, a, b, c, d, are in the same proportion

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portion when $\frac{a}{b}=\frac{c}{d}$, and any number of quantities, a, b, c, d, e, f, &c., have the same proportion when $\frac{a}{b}=\frac{c}{d}=\frac{e}{f}$ &c.

Hence we have a criterion by which proportional quantities may easily be distinguished, viz. an equality of ratios; and this being understood, the whole doctrine of proportion flows immediately from the above obvious principles.

Four proportional quantities are commonly expressed by saying that a is to b as c to d, and they are usually written thus, $a:b::c:d$; where b and c are called the *mean* terms, and a and d the extremes; also a and c are called antecedents, and b and d their consequents. The subject is further illustrated in the following articles:

Article I.—When four quantities are proportional, the product of the two means is equal to the product of the two extremes. For since, by hypothesis, $\frac{a}{b}=\frac{c}{d}$, multiply both sides of the equation by bd, and we have $\frac{abd}{b}=\frac{bcd}{d}$, or $ad=bc$.

Also, conversely, if the product of any two quantities be equal to the product of two others, the four quantities are proportional. For, since $ad=bc$, divide by bd, and we have $\frac{ad}{bd}=\frac{bc}{bd}$, or $\frac{a}{b}=\frac{c}{d}$; that is, $a:b::c:d$.

Article II.—If four quantities are proportional when taken *directly*, they will be proportional when taken *inversely*; that is, if $a:b::c:d$, then will $b:a::d:c$.

For when $a:b::c:d$, we have $\frac{a}{b}=\frac{c}{d}$, and dividing unity by each of these ratios, or inverting them, we get $\frac{b}{a}=\frac{d}{c}$, or $b:a::d:c$.

Article III.—When four quantities are directly proportional, they will also be proportional when taken alternately; that is, if $a:b::c:d$, then will $a:c::b:d$.

For, because $\frac{a}{b}=\frac{c}{d}$, multiply both sides by $\frac{b}{c}$, and we have $\frac{ba}{bc}=\frac{bc}{dc}$, or $\frac{a}{c}=\frac{e}{d}$; that is, $a:c::b:d$.

Note.—Here, unless the four quantities are all of the same kind, the alternation cannot take place.

Article IV.—When four quantities are proportional, the first together with the second is, to the second, as the third together with the fourth is to the fourth; that is, when $a : b :: c : d$, $a + b : b :: c + d : d$.

For, since $\frac{a}{b} = \frac{c}{d}$, add unity to each side, and $\frac{a}{b} + 1 = \frac{c}{d} + 1$, and reducing each to an improper fraction, $\frac{a+b}{b} = \frac{c+d}{d}$, that is, $a + b : b :: c + d : d$.

Article V.—When four quantities are proportional, the excess of the first above the second is, to the second, as the excess of the third above the fourth is to the fourth; that is, when $a : b :: c : d$, $a - b : b :: c - d : d$.

For, since $\frac{a}{b} = \frac{c}{d}$, we have $\frac{a}{b} - 1 = \frac{c}{d} - 1$, or $\frac{a-b}{b} = \frac{c-d}{d}$, that is, $a - b : b :: c - d : d$.

Article VI.—When four quantities are proportional, the sum of the first and second is, to their difference, as the sum of the third and fourth to their difference; that is, when $a : b :: c : d$, then will $a + b : a - b :: c + d : c - d$.

For, by Art. IV., $\frac{a+b}{b} = \frac{c+d}{d}$, and by Art. V., $\frac{a-b}{b} = \frac{c-d}{d}$; multiply both equations by $b\,d$, and $d \times (a + b) = b \times (c + d)$, also $d \times (a - b) = b \times (c - d)$; and if we divide equals by equals, the quotients will be equal; therefore $\frac{d \times (a+b)}{d \times (a-b)} = \frac{b \times (c+d)}{b \times (c-d)}$; that is, $\frac{a+b}{a-b} = \frac{c+d}{c-d}$, or $a + b : a - b :: c + d : c - d$.

Article VII.—When three quantities are proportional, the first is, to the third, as the square of the first to the square of the second; that is, if $\frac{a}{b} = \frac{b}{c}$, or $a : b :: b : c$, then will $a : c :: a^2 : b^2$.

For, since $\frac{a}{b} = \frac{b}{c}$, we have $ac = b^2$: multiply by a, and $a^2c = ab^2$; or, $\frac{a}{c} = \frac{a^2}{b^2}$ by Art. I., therefore $a : c :: a^2 : b^2$.

This proportion is often used in Dynamics.

Article VIII.—When any number of quantities are proportional, as one antecedent is to its consequent, so is the sum of all the antecedents to the sum of all the consequents; that is, if $a : b :: c : d :: e : f$ &c., or $\frac{a}{b} = \frac{c}{d} = \frac{e}{f}$, &c., then will $a : b :: a + c + e : b + d + f$.

For, since $\frac{a}{b} = \frac{c}{d} = \frac{e}{f}$, &c., we have $ad = bc$, also $af = be$, and by adding equals to equals, $ad + af = bc + be$; add ab to both sides, and $ab + ad + af = ba + bc + be$, or $a \times (b + d + f) = b \times (a + c + e)$; that is, (Art. I.) $\frac{a}{b} = \frac{a+c+e}{b+d+f}$, or $a : b :: a + c + e : b + d + f$.

Article IX.—When four quantities are proportional, any like powers or roots of these quantities will be proportional: that is, if $a : b :: c : d$, then will $a^m : b^m :: c^m : d^m$; where m may be either a whole number or a fraction.

For, since $\frac{a}{b} = \frac{c}{d}$, therefore $\frac{a^m}{b^m} = \frac{c^m}{d^m}$, or $a^m : b^m :: c^m : d^m$.

Article X.—If the corresponding terms of two ranks of proportional quantities be multiplied together, their products will be proportional; that is, if $a : b :: c : d$,

and $e : f :: g : h$,

then will $ae : bf :: cg : dh$.

For, since $\frac{a}{b} = \frac{c}{d}$, and $\frac{e}{f} = \frac{g}{h}$, multiply equals by equals, and $\frac{ae}{bf} = \frac{cg}{dh}$, or $ae : bf :: cg : dh$; and the same is true for any number of ranks of proportionals.

Article XI.—When four quantities are proportional, if the antecedents or consequents be multiplied or divided by any quantity, the products or quotients will be proportional; that is, if $a : b :: c : d$, then will $ma : mb :: nc : nd$.

For $\frac{a}{b} = \frac{c}{d}$, therefore $\frac{ma}{mb} = \frac{nc}{nd}$, or $ma : mb :: nc : nd$; where m or n may be either whole numbers or fractions.

Article XII.—If there be four quantities, such that $a : b :: c : d$, and four others, such that $c : d :: e : f$, then will $a : b :: e : f$.

For, since $\frac{a}{b} = \frac{c}{d} = \frac{e}{f}$, therefore $a : b :: e : f$.

The above articles contain nearly all that is necessary to be understood concerning proportional quantities; and by students who know how to manage a simple equation in algebra, they will be read without much difficulty in a very short time. Not being incumbered with equimultiples, the demonstrations are general, and will serve equally for either commensurable or uncommensurable quantities. Very little indeed of what I here send you can be said to be entirely new: it is presumed, however, that the principles on which the above demonstrations are founded, are laid down with more clearness and precision than in any author who has written on this subject. By inserting them in your valuable Miscellany, you will very much oblige your very humble servant,

Wm. Marrat.

Boston,
May 18, 1809.

LXXVI. *A few Hints concerning the Benefit that may be expected from the Nature of Coal Gas.*

To Mr. Tilloch,—Sir,

The gas which is obtained when coal is distilled in close vessels having lately attracted the attention of the public, chiefly on account of its application for the production of artificial light, has encouraged me to lay before your readers a few observations concerning this subject, which bids fair to be ranked among the most beneficial applications of chemical science to the useful purposes of society. I will leave it to your judgement to abridge of these lines, or to cancel, whatever you deem unworthy of notice, to make room in your journal for more valuable subjects:

you may have received from other quarters. The brilliancy of the light which is produced during the combustion of *coal gas*, is so superior in splendour and beauty, that it surpasses not only wax candles and the best spermaceti oil, but every other substance hitherto employed for artificial illumination. The coke obtained in the same process is so valuable, that it appears inexplicable that men should not avail themselves of this mode of procuring light, to the almost total exclusion of all other methods now in use. As a landholder, placed among an industrious but wholly illiterate society of men, I have had the more opportunity of trying this species of fuel or coke, which I could not otherwise procure in this sequestered spot, at a tolerably cheap rate, for purposes to which it has not, as far as I know, been hitherto employed. I must tell you that I am my own *lime-burner, plaster* (of Paris) *baker*, and *brick-maker;* and that in these processes of rural œconomy I have derived the greatest benefits from this species of fuel, which I now prepare at a cheap rate, although I waste almost the whole of the light of the coal gas intentionally. The coal which I employed formerly for the burning of limestone into lime is a very inferior kind of small coal, called here *Welsh culm;* the only kind of limestone I can command is the gray kind, which strongly effervesces with acids. It readily splits into distinct layers, and becomes perfectly white after having been exposed to a red heat. The kiln for burning it into lime is a cup-shaped concavity, surrounded with solid brick-work, open at the top, and terminating below by an iron grate. It has a stone door that may be opened and closed for charging and emptying the furnace when required. This furnace I formerly charged with alternate strata or layers of small coal and limestone, the latter being broken previously into pieces not larger than a man's fist, until the kiln was completely filled. The stone is thus slowly decomposed; the upper part of the charge descends, and when it has arrived at the bottom of the furnace new strata are super-imposed, so as to keep the furnace continually full during a period of 50 hours. The quantity of lime I thus procured with small coal formerly amounted to 85 bushels. The strata of coal necessary for the production of this quantity of lime require to be four inches thick, and the time absolutely necessary for the process of calcination was, as stated already, 50 hours. On applying coke instead of coal, (which coke I obtained from the same kind of coal,) the produce of lime may be increased to nearly 30 per cent. from the same furnace, and the time required to effect the calcination of this quantity of limestone is reduced to 39 hours: it also requires less attendance and less labour, and the whole saving thus accomplished amounts to more than 50 per cent. on the lime-kiln. I have lately also employed coke for the burning of bricks. My bricks are burnt in *clamps* made of bricks themselves. The place for the fuel, or fire-place, is perpendicular, about three feet high. The flues are formed by *gathering* or arching the bricks over, so as to leave a space between each of a brick's breadth; and as the whole of the coal, if this fuel be employed, *must*, on account of the construction of the pile, be put in at once, the *charge* of the bricks is not, and never can be, burnt properly throughout; and the interference of the legislature with regard to the measurement of the clamp is a sufficient inducement for the manufacturer to allow no more place for coal than he can possibly spare:—the reason is obvious. If coke be applied instead of coal, the *arches* or empty spaces in the clamp or pile, as well as the strata of the fuel, may be considerably smaller: the heat produced in this case is more uniform and more intense, and a saving of 32 per cent. at least is gained. In the baking of *my own plaster stone* (the compact sulphate of lime of a reddish tinge) I also employ coke. The calcination of this stone for manure I perform in a common reverberatory furnace, and the men who conduct the process (who are otherwise averse to every thing new) are much pleased with the steadiness of the fire, and little attendance which the process requires, when coke is used instead of coal. These are the few facts I wish to state to you, with regard to the useful application of this species of fuel, which, no doubt, hereafter will become an object of œconomy of incalculable advantage to individuals, if its nature be better understood

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than it is now. In reading the ingenious observations of Mr. Accum * with regard to this subject, who states that the effects of different kinds of fuel may be learnt from the time required to heat a given quantity of water, &c., the determining, from the quantity of fuel consumed, the œconomical application of different sorts of combustible matters, to ascertain the cost of the one when compared with the cost of the other, particularly arrested my attention with regard to the œconomical application of coke, when compared with fuel of other kinds now in use.

It will not, I am persuaded, be regarded as indicating a wish to detract in the smallest degree from the well-earned credit due to the talents and skill of this chemist, so respectably known to the scientific public, when I venture to state that I am inclined to believe he has rather over-rated the power of coke, in stating it as three to one †, unless his coke be considerably better than mine obtained from Welsh culm. My experiments in the rough way gave about two to one, comparing it with coal weight for weight. I have also no doubt that coke might be advantageously employed in the smelting-houses. 14 pounds of brass can be fused in a portable chemical furnace, by means of coke, in 48 minutes, which, with compact wood charcoal, I could not accomplish in less than 1 hour and a quarter. The difference with regard to price in this case is very remarkable. When the coke made from Welsh culm is once completely ignited, (which it readily is, if not decarburetted too much,) it throws out a very compact and steady heat, and yields but a very slight sulphureous odour; and this ceases when it is fully ignited. It lasts a longer time in a state of ignition than charcoal of wood in a quadruple proportion, and its heat is constantly equal, and of almost the same intensity. It also requires less trouble and attendance.

Having stated above, (and as is indeed well known,) that the brilliancy of the light produced during the combustion of *coal gas* is far superior to oil or candles, and being desirous to know to what this preeminence was owing, I made a number of experiments, of which I shall merely state the results, and not the proceedings. The gas was prepared from Welsh culm enclosed in a common iron pot covered with a head made of *brick ware*; the tubes for conducting the gas were leaden pipes furnished with perforated *roses* like the extremity of the pipe of a common watering-pot. In this manner it was found that the degree of the illuminating power of the coal gas differs according to the degree of heat employed for its evolution. Coal exposed to a dull red heat, just sufficient for the production of the gas, yielded a gaseous product, which exhibited much less brilliancy when burnt than gas obtained during a temperature of a bright cherry redness.

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* The Report, &c., page 31.   † Ibid.

100 cubic inches of the former gas when made to burn slowly from a small aperture under a gardener's large glass bell, connected with a stone barrel filled with oxygen gas, required 259 cubic inches of oxygen for its complete combustion. On removing the residual gas into a stone pan containing a ley obtained from the ashes of brush-wood, 114 cubic inches of the gas vanished.

100 cubic inches of coal gas obtained at a cherry red heat, required for its combustion 312 cubic inches of oxygen gas obtained from oxymuriate of potash. The volume of gas, after having been agitated with a like alkaline ley, lost 117 cubic inches. Hence the light of this gas, or its intensity, is probably in the ratio of the quantity of oxygen necessary for the combustion of the carburetted hydrogen. An *increased* temperature produces a gas better adapted for illumination than a gas procured by a degree of heat merely sufficient for its evolution.

The gas obtained at a *low* temperature has a much stronger odour than that produced during an *increased* temperature. It contains a considerable portion of sulphuretted hydrogen; for on collecting a quantity of it in a gasometer made of two puncheons, the one of which was painted with white lead within, and suffering the gas to stand in this apparatus, it completely blackened the white paint of the wooden vessel. The gas obtained at an increased temperature acted but feebly on the oxide of lead.

On passing a stream of the gas obtained from Welsh culm through a solution of acetite of lead with excess of acetous acid, it instantly produced a copious black precipitate, which effervesced strongly with nitric acid, and yielded much sulphuretted hydrogen gas.

A quantity of gas which had been repeatedly agitated in contact with a solution of acetite of silver with excess of acid, when suffered to burn under a gardener's bell-glass filled with common air, deposited a dew on the inner surface of the bell; the moisture produced permanently reddened a blue cabbage leaf, and caused a precipitate in acetate of barytes; the precipitate was insoluble in dilute nitric acid; the gas therefore still contained either sulphureous or sulphuric acid; and hence the gas, when intended for the illumination of dwellings, should be procured at a bright red heat. The odour of it becomes under this condition diminished, and the property of blackening paint is but slight and feeble.

The method recommended to deprive the gas of its odour, by passing it through lime-water, or through lime in a state of ignition, was found to be absolutely insufficient; continued agitation with lime diffused through water to the consistence of cream, with a portion of alkaline ley, was found to deprive the gas of part of its strong odour.

If the gas from coal be suffered to stand over water for some days, or if it be agitated with that fluid, its property of producing a brilliant white light is much impaired. It then burns with a blueish flame instead of a white one hence, for the purpose of illumination, the gas should be used as it issues from the distillatory vessel.

Whether the sulphuretted hydrogen gas spoken of is an accidental or unavoidable ingredient in the gas of coal, I have not been able to learn. Indeed the whole nature of the gas seems to be but ill understood by chemists themselves. For, notwithstanding the merit which Mr. Henry's paper in the Philosophical Transactions for 1808, p. 11, describing an apparatus for the analysis of the compound inflammable gas, contains, the results of his experiments cannot be accurately relied on. The imperfection of the apparatus employed by this philosopher must be obvious to every one; for it cannot separate nor ascertain the nature of above one-fifth of the inflammable gas from coal, the composition of the remaining portion being left still undetermined.

The quantity of gas obtained from one chaldron of Welsh culm, I have reason to state, is sufficient to give a quantity of light equal in intensity to that afforded by 869 tallow candles, eight to the pound, burning for 16 hours. The increase of coke in bulk is more than 50 per cent.

The quantity of tar and other condensable products I have not been able to ascertain, owing to the imperfect state of my apparatus. They appear to amount at least to one-tenth of the coal employed. They will probably be found useful hereafter. For cart grease, or to paint palings, &c., the tar may certainly be used with advantage.

With the pungent ammoniacal saline liquor I have made no rustic experiment.

J. W. Davis.

Tanbymoore, May 1, 1809.

LXXVII. *On the fertilizing Properties of Manures which contain Ammonia. By Mr.* William Cox*.

It is only within the last three weeks or a month that any application was made to me for my opinion on the use of any of the articles produced by Mr. Winsor's apparatus, and therefore I have not had the opportunity of making any experiments with this identical ammoniacal liquor, especially with regard to the use to which I beg leave to state I think it applicable. But it will readily be admitted by several gentlemen present, acquainted with chemistry, that ammonia is a chemical matter, as identical in one situation as in another; and as long as I am certain that it is ammonia, it is the same thing if it is produced here or in the North. I was desirous, some years ago, of making ammonia in large quantities, and was of course at some pains in ascertaining what would be the most œconomical process for its production: I had resort to the various different articles of a refuse nature, such as were easily to be procured at the cheapest rate, for the purpose of obtaining ammonia; but I found, however much I varied the article, that, according to the quantity of ammonia I could produce from that article, the farmers had always been beforehand with me in raising the price of it; as they found it useful in the same proportion in its application on land: and that is as far as I am acquainted with the matter. Of course the production of ammonia, on a great scale, would be of great importance to the agricultural interest.

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* From Minutes of Evidence taken before the Committee of the House of Commons on the Gas-Light and Coke Company's Bill.

Are you acquainted with the subject of the use of ammoniacal liquor, in agriculture?—I am.

[The witness here read a paper to the committee.]

Gentlemen,

Your inquiry as to my opinion of the uses to which your ammoniacal liquor is or may be applicable, I shall endeavour to satisfy as far as the shortness of time will permit. Positive evidence of the immediate result of experiments, which require time and seasons, cannot be obtained in regard to such uses as, from the closest analogy and the most reasonable inference, we should be induced to apply this production of your process to. There are many uses in the arts and manufactures to which the application of the ammonia or volatile alkali is well known, and which are already in part enumerated. But when the demand for these purposes is supplied, and, on the probable great extent of the production of your ammonia, should a surplus quantity remain, I have reason to think, that in some very considerable departments of agriculture that surplus, however great, will find a ready and adequate market. A judicious application of ammonia to land before it be sown with turnips, (but if afterwards, on no account after the plants are up,) is likely to produce the most beneficial results. What justifies me in this conclusion, is the simple consideration, that all the powerful and concentrated manures of high price, and in great request, are just so in the degree in which I have found them by analysis to contain either ammonia or the elements that compose it. Soot, well known to be in small quantities a powerful encourager of vegetation, contains much carbonate of ammonia, combined with some of the carbonaceous parts, rendering them extractive and soluble in water, forming a brown pungent liquid. Pigeon dung is a dressing for turnip land in great request in the North, where many hundred quarters are annually sold at 121*s.* the quarter, though a very small proportion of the demand is supplied. I have found, by experiment, that this material is richly impregnated with carbonate of ammonia as well as with the well-known element of ammonia, azote, which, in the natural decomposition of the manure by putrefaction, when committed to the earth, will be produced. Rape dust is that particular part of the seed (left after the oil is pressed out) which is intended by nature to corrupt, and become the early cause or stimulus of the growth of the embryo germ, and therefore contains the same element, and which we can readily, by a chemical process, exhibit in the ammonia which rape dust may be made to yield. It is hardly necessary to mention urine, &c., from which ammonia is obtained in great quantity, or the dung of all animals, which contains the same principle. It was from the dung of the animals which fed on the fertile plains of Egypt that all the sal-ammoniac known in commerce was for many centuries obtained. From that country, the site of the temple of Jupiter Ammon, its name is derived. Soon after sal-ammoniac became an article of European manufacture, it was discovered that the bones and horns of animals yielded its peculiar salt, that is to say, the ammoniacal principle, in much greater quantity than their dung, and those parts were alone used to the exclusion of these: hence the name spirit of hartshorn, given to the volatile alkali used in medicine. It has been of late years discovered, that the scrapings, shavings, and chips of the horns used in manufactures (particularly of the knife handles at Sheffield) are the most powerful and the best of all land dressings known; and it is from these very materials also that the greatest quantity of ammonia is to be obtained, wool, silk, and hair excepted, and these are again in great use in agriculture, when collected and sold as old woollen rags. Bones of all kinds, not excepting human bones, are sent by sea in great quantity from this metropolis into the North; many hundred tons of these are ground, or rather broken small, in mills contrived on purpose, as the quantity necessary for an acre of land is small in comparison of other materials. The convenience of easy carriage is the cause of the most distant lands being brought into the richest cultivation. It would not be proper, on this occasion, to enter into a theoretical disquisition on the nourishment of vegetables, whether they derive their food wholly, or only in small part, from the earth by their roots, or from the atmosphere by their leaves and green parts; but it appears clear to me, that that principle which the farmers term warmth and force, is constantly accompanied by the chemic element mentioned. This stimulus of encouragement and force is of more consequence to the growth and eventual vigour of annuals than of perennials, and particularly at the early periods immediately succeeding the expenditure of this sure principle which nature has provided in the seed. The putrefactive fermentation always generates ammonia; the earth imbibes the different miasmata, and holds them in store for the use of plants; to these they impart health, strength, and, as may be said, appetite.

A great difference is observed by farmers in the qualities of the manure of cattle, when fed on oil cake or on hay; it is supposed to be of four times the value in the first case. The beneficial effects of sometimes mixing lime with arable soil is easily explained in this way. The ammonia is always to be recognised by its peculiar smell. As soon as newly slacked lime is mixed up with the mould of a good soil, but which is beginning to show signs of impoverishment, in this case, the ammonia, which had formed a chemic combination with the fixed acids of the manure (formerly ploughed in and fermented) is set at liberty. These are the phosphoric and vitriolic acids, which, as is well known, will leave ammonia to combine with lime. I have therefore no hesitation in declaring, as matter of opinion, that the production of ammonia, in great quantity, and its judicious application to agricultural purposes, are processes of very great importance to the landed interest.

LXXVIII. *Geological Observations on the Excavation of Valleys, and local Denudations of the Strata of the Earth in particular Districts, &c., in Reply to Mr.* JOHN CARR's *Letter in the last Number, p.* 385. *By Mr.* JOHN FAREY.

> "It is only from the itinerant geologist cautiously pacing over various and extensive districts, and marking with experienced intelligence the wonderful phænomena which every where present themselves, that we can hope to obtain that accumulation of practical facts which can alone guide us to a sober and correct theory of the natural causes which, at remote periods, have operated those stupendous changes which are every where seen on and near the surface of our globe*."
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> JOHN CARR.

To MR. TILLOCH,—SIR,

A WET morning, which seems to threaten an interruption of some hours' duration, to my researches in the highly interesting district which surrounds this town (Sheffield in Yorkshire), presenting the opportunity of looking into the last Number of the Philosophical Magazine, I have been induced to trouble you with a few lines, in explanation of some points, which your able correspondent Mr. John Carr of Manchester has touched upon in his Letter therein. In the present state of geological knowledge, nothing can be more just than the introductory remark of Mr. Carr, which I have quoted above, as to the class of persons who are at this juncture most likely to trace out and discover the natural *causes* which have operated, in effecting the present state and condition of the earth; arising from the circumstance, that very few, of the great and leading *facts* appertaining to the crust of the earth have yet been touched upon, much less have they been so fully treated on or illustrated by geological writers, that "the closet geologist" can, over an "accumulation of practical facts," sit down, and deduce "a sober and correct theory," or even judge of the truth or otherwise, of suggestions as to causes, which "itinerant geologists" may advance. This state of things ought not, however, longer to continue, but the exertions of every well-wisher to science ought to be redoubled, in tracing out and *fully describing* the facts which the British strata present; were it only for securing to this country the honour of perfecting, and rendering the important discoveries of Mr. William Smith relative to the strata and their alluvial ruins, as subservient to science as they have already begun to be to the mining and other œconomical interests of our countrymen, in different counties.

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* Vide p. 385.

Wonderful and important as the discoveries made about 17 years ago have proved, as to the certain *order* and *continuous planes* of the undisturbed strata, each of which, almost, entombing its own peculiar *remains of organized beings;* and the more recent results, that all such are perfectly *distinguishable from the present race* of animals and plants occupying the surface of the strata, as well those parts of them which are now sub-aqueous, as the others: yet, without the means of accurately distinguishing *alluvial* or moved matters, from the stratified sand and other substances, which have hitherto been almost universally confounded with them; of discriminating between the *extraneous* and the *local* alluvia of any district; and further acquiring the knowledge, that *faults* or fissures, slips, throws, or whatever else they may be denominated, are the mere fracture and displacement of the piles of strata, and do not in any instance affect the order or nature of the strata, (beyond their immediate vicinity,) when an extent of the series is taken into account, commensurate with the derangement that the two parts of the separated pile of strata have sustained, and the effect of a subsequent denudation or excavation of the surface to its present diversified form of hills and valleys is duly considered: without, I say, that these latter facts and their practical applications were also known and made, the order and continuity of the strata might still have remained as unproductive to geological science, or even to scientific mining, as they have hitherto proved in the hands of practical colliers and ironstone miners, who have been fully aware of these two points, nay all their operations have been founded upon them, from the earliest periods in the practice of their arts: but unfortunately, with some rare exceptions, such have confined their researches to their own particular fields or to very limited districts round them, and have too often satisfied themselves, with the most vague and unsatisfactory guesses, at the identity of the particular strata worked by the miners in other districts, even of those which adjoin, in numerous instances: and I am sorry to add, that after visiting more than 200 collieries between Nottingham and this place, and conversing with the owners or workmen or both, upon most of them, I have not been able yet to discover any two whose ideas are perfectly consistent with each other, as to the identity or otherwise, of the coal and ironstone working in places distant from their own works: my confidence, however, increases daily, of being able (should health and prudential considerations admit of a sufficient application of time to the subject) to reconcile all the facts I have collected, to the satisfaction of this great body of practical men, and to scientific inquirers in general. The zeal which I feel, for stimulating others, better qualified than myself, to enter on and pursue similar and even more minute inquiries in this or other districts, than I have been able to accomplish or attempt, has however here led me beyond what I intended at present: and I return to page 385 of your last Number, in order to notice the opinion advanced by Mr. Carr, that "solely from the *superior durability* of its materials, which have withstood the operation of those tremendous agents that have swept away the surrounding country," are we to look for the cause of an isolated mountain composed of successive piles of strata: in order, on a point of so much moment to mention, that though a gritstone rock very often occupies the very summits of hills in the coal districts, yet that such are generally too soft and their blocks adhere too slightly, to admit of our referring the *form* of the hill *solely* to the resistance these rocks offered either to a violent action of gravity from above, (as I have supposed,) or to the sudden or even to the long-continued action of "water," moving over the surface according to any law which I have been able to imagine: besides, to what part of the earth's surface is the abraded matter removed? The very extensive denudated district which I am now in, furnishes a quantity of local alluvia inconsiderable indeed; all that I have yet seen, would not, if added together, amount to the thousandth part of the quantity removed from a single mile in length, of each of a hundred different excavated valleys, which I could refer to on my map, and the little that there is, is almost invariably found so near to the present currents of the rivers and brooks, as to be naturally enough referred to the torrents which hurry through these valleys in ordinary heavy rains; not to mention the bursting of water-spouts &c. which we are at liberty also to suppose may have occurred, on the hills above. The hummocks of *gravel* in Derbyshire which I have mentioned page 261, as well as the immense tract of sandy gravel on Sherwood Forest in Nottinghamshire, belong to the extraneous alluvia, and contain no pebble or stone, wherein a high degree of *rounding*, does not concur, with its chemical qualities, in proving the distance it has travelled, to its present resting-place. I cannot but entirely dissent from the opinion adopted by Mr. Carr, at the top of the next page, viz., that the terrestrial *strata* "could *only be derived* from the destructive transportation of *other strata* equally extensive; and the present elevation of stratified mountains is demonstrative evidence of the countries which, in disappearing, have furnished such vast masses of diversified materials for the formation of *other* stratified countries in other situations," because, such ideas have been often promulgated, and found so "utterly incongruous" with the phænomena which the strata themselves present, both in the regularity of their planes, and the lodgment of perfect and peculiar organized remains in them, respectively, that I cannot but consider it, "at once matter of surprise and regret," that Mr. Carr should have compared this exploded notion, with the *principle* of *gravitation*, as elucidated by Newton, T. Simpson, and La Place, to the entire accounting for, all phænomena, to which it has been legitimately applied.

From what source can it be inferred, that the "*present* laws of Nature," as Mr. C. has defined them, have been *in action millions of years?* any more, than that the fanciful creation of an "*erratic planet*" is intended "for accomplishing, *almost in an instant*, that which, far more probably, required many thousands of years to effect?" My personal acquaintances well know, that I have all along supposed the probable period to be very long; during which a *satellite* revolved in a continually decreasing orbit (from causes that are perhaps assignable, without clumsily cutting any Gordian knot) and effected the stupendous operations on the strata, previous to its fall into, and assimilation with, the compound mass of the terraqueous globe, which it is the business of "closet" as well as "itinerant" geologists to investigate and understand fully, before they pronounce the proposer of a new application of the principle of gravitation to the earth, as a person forsaken by the genuine spirit of philosophy, and become "bewildered in the unprofitable maze of hypothesis!" Surely *gravity* (as exemplified daily by the tides, p. 259,) may safely be classed among the "present energies" of Nature: and who is there, that cannot perceive, that the *energies*, be they what they may, which effected the disruption and denudation of the strata, must have acted in degrees and modes, utterly distinct from those which have prevailed, since the last and great operations of creative power were performed, in the creation of the present race of vegetables and animals, and of *man*, whose reasoning faculties rendered him capable of tracing back to events, which took place long, very long prior to the existence of his species.

Strong as the language I have used herein may at first seem, I will not anticipate the slightest irritation in Mr. Carr thereat, much less an explosion of any thing like virulence on his part, but on the contrary, cordially wish, that he would engage seriously in applying the *action of water*, (without the reversed and deranged action of gravity) to the explanation of the phænomena presented by the neighbourhood of Manchester, or any other which he may choose, and convey to us, both the facts and the reasonings, through the medium of your useful Magazine: recollecting, that the *tides* which I have supposed, necessarily imply all the mechanical agency of water, which is consistent with the periodic time and circumstances of the satellite occasioning them and giving impulse to the fluid; and Mr. Carr will not, I hope, fail to consider, and inform us, of the motive forces propelling and directing his "incessant operator" in the excavation of valleys. The example of the immortal *Newton*, in declining to attempt the definition of the cause of gravity, to which neither he nor any one else have yet found themselves equal, shall be my excuse, in not attempting a conjecture on the manner in which the forces *were directed*, which excavated the valleys: the facts of many of them having been mechanically excavated, and that none (comparatively speaking) of the displaced materials are now any where to be found, are, as I think, incontrovertible.

Perhaps Mr. Carr, when he speaks of water having given "mobility and transportation to such massive and diversified materials," as compose the strata, had not contemplated a supposition, which forced itself upon my mind, after having perceived the difficulties which his position involves, viz., that each successive stratum in ascending the series, was *created* since the animal or vegetable remains which it covers, had completed their growth, and the deposition of which stratum, or its precipitation from the superincumbent fluid, perhaps, occasioned the successive extinction of these organized beings. The universal prevalence of *grains* in siliceous strata, suggests the supposition, as I think, that something analogous to the formation of *hail* in a storm, (in irregular crystals) took place during the precipitation of *silex*, generally, in very minute grains; but in the first or Mill-stone Grit Rock (vol. xxxi. Plate II.) it is not uncommon to meet with grains half an inch, and even in some cases three quarters of an inch in diameter, having that smoothness of surface, as to induce Mr. Whitehurst and many others to describe these large siliceous grains as *rolled pebbles*, but which opinion I never could see reason for adopting; the surface of these large grains, appearing to me, no way different from the surface of the smallest siliceous grains, when viewed by a magnifier proper for showing each under the same apparent angle. I am, sir, your obedient servant,

Commercial Inn, Sheffield,
June 6, 1809.

JOHN FAREY.

LXXIX. *Contrivance for preventing Doors from Dragging on Carpets.* By Mr. JOHN TAD*.

SIR,

I HAVE taken the liberty of laying before the Society a model of my invention to prevent doors from dragging on carpets, and to keep out the current of cold air, which enters under such doors as are not close to the carpets underneath them.

I can affix this machinery to the bottom of any door, so that the door shall pass over the carpet with ease, and, when shut, be air tight. It obviates the necessity of screw rising hinges, and is less expensive than other inventions for the same purpose.

The machinery is constructed of a slip of well seasoned beech wood, equal in length to the width of the door; this slip is one and a quarter inch wide, and half an inch thick, and to be covered with green cloth on the inside; it is to be hung to the bottom of the door with three small brass hinges, and is drawn up by a concealed spring as the door opens, and is forced down when the door shuts, by one end of it, which is semicircular, pressing upon a concave semicircular piece of hard beech wood, fastened at the bottom of the door case, and which holds it down close to the floor or carpet, so as to exclude the air from entering under it. Hoping this invention will meet with the approbation of the Society, I remain, with respect,

Sir, your most humble servant,

No. 4, Little Hermitage Street, Wapping,
Nov. 24, 1807.

JOHN TAD.

To C. TAYLOR, M.D. Sec.

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* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1808.——Five guineas were voted to Mr. Tad for this communication.

Reference

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Mr. J. Tad's method of causing a Door to open over a Carpet.

Fig. 1.

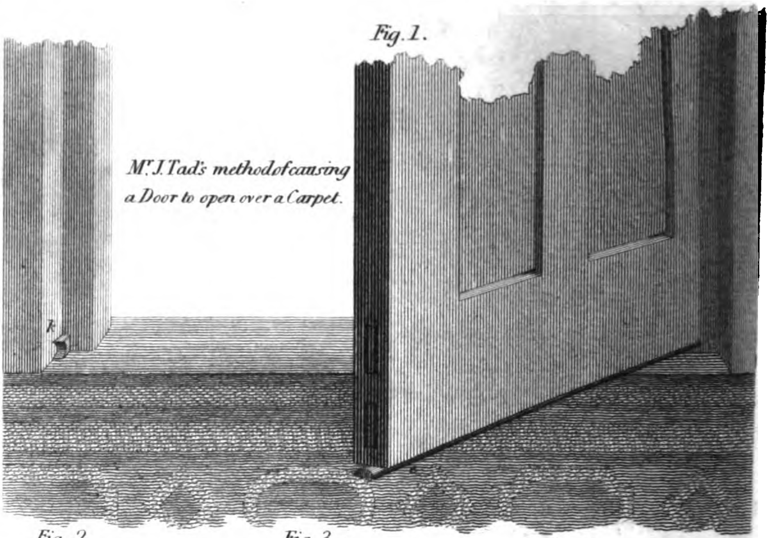


Fig. 2.

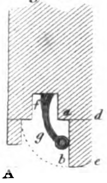


Fig. 3.

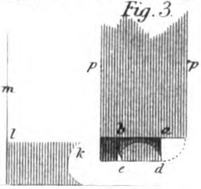
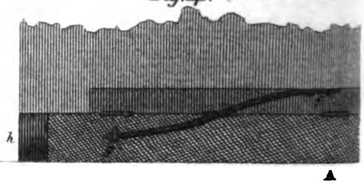


Fig. 4.



Mr. W. Barlow's Wrench, for Screw Nuts of any size.

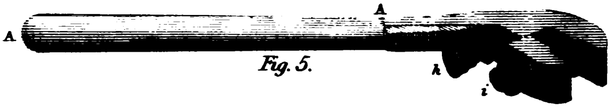


Fig. 5.

Section.

Enlarged view of the Head.

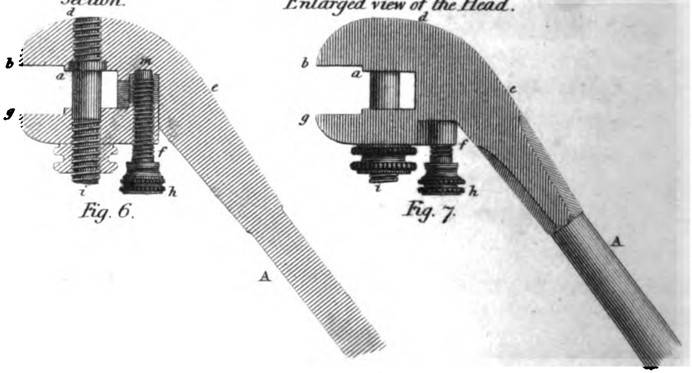


Fig. 6.

Fig. 7.

S. Porter sculp

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Reference to Mr. TAD's *Method of preventing Doors from Dragging on Carpets.* See Pl. XIII. Figs. 1, 2, 3, *and* 4.

Mr. Tad's invention consists in first cutting away the bottom of the door, so that it is about one inch and a quarter above the floor; this allows a sufficiency of room for the door to open over any carpet. To close the opening which would now be left under the door when shut, he proposes to fix beneath the door, by means of hinges, a slip of wood, of which *a b d e*, figs. 2 and 3, Plate XIII. is a section. Fig. 1 is a perspective view of the bottom of a door, with the invention annexed to it; fig. 2 is a section across the door when closed; fig. 3 is a view of the edge of the door when open; and fig. 4 is a section supposed to be made by cutting the door in two parts, edgeways. The hinges, on which the slip turns, are fixed to the edge. In figs. 2 and 3, from *a* to *b*, is exactly one inch and a quarter, so that when the ruler is turned down upon the hinges, it reaches the floor A A as in fig. 2; in the other direction *a d* it is much less, being only half an inch, so that when it is turned up under the door, as in fig. 3, it leaves three quarters of an inch clear of the floor. It now remains to show how the ruler is turned up or down:—it has always a tendency to rise up into the state of fig. 3, by the action of a steel wire spring, shown in figs. 2 and 4, which is concealed in a rebate cut in the bottom of the door; one end of the wire is screwed fast to the door at *f*, the other is inserted into an eye fastened into the slip at *g*, to throw it down into the position of figs. 2 and 4. The end *h*, fig. 4, of the slip furthest from the hinges of the door, is cut into a semicircle, as seen in fig. 3. When the door is just closed, this semicircle is received into a fixed concave semicircle *k*, fig. 3, cut in the end of a piece of wood *k l*, made fast to the door-case; the line *m l*, fig. 3, represents the plane of the door when shut, and *p p* part of the door seen edgeways: as the door in shutting moves from *p* to *m*, the semicircular end of the slip *a b d e* presses against the end of the piece *k l*, and as the door proceeds, it turns down as in fig. 2, so that by the time the door is shut, the slip is turned quite down; the edge *e b* of the slip is cut into a segment of a circle struck from the hinges on which it turns. The perspective view in fig. 1 shows that this contrivance, applied to any door, will not offend the eye, as it can scarcely be distinguished from an ordinary door. *K*, fig. 1, shows the concave semicircle of the piece of wood fastened to the door-case, in which the semicircular end of the slip *e* is to be received.

LXXX. *Description of an Improved Screw Wrench to fit different-sized Nuts or Heads of Screws.* By Mr. WM. BARLOW, *of His Majesty's Dock Yard, Portsmouth*[*].

SIR,

PERMIT me to make a few observations on a shifting screw wrench of my invention, which I beg leave to lay before the Society of Arts, &c., through the hands of Mr. Brunel, inventor of the block machinery here.

I have found, from long experience, the imperfections of the various wrenches in common use, for the screw heads and nuts of engines in general, which are often materially injured for want of an instrument which would fit variety of sizes, and be applied with as much advantage as a solid wrench. I have had it in view to unite steadiness with conveniency in making such an instrument; and flattering myself that I have obtained both, I am desirous to communicate my invention to the Society, and have therefore sent an instrument on the principle I have actually used, and which has met with the approbation of my employers and other persons.

This wrench, by means of a nut and screw, is adjusted with the greatest ease to the exact size required, and in that state rendered so steady that in use it is found equal to a solid wrench.

I have, for several years, been intrusted with the care and repairs of many valuable engines of various descriptions, composing the block machinery in this dock-yard, and I have always considered it as an object of great importance, for the preservation and neat appearance of engines, to attend to all the means which would obtain these advantages, and such, I think, will arise from the use of my universal wrench.

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[*] From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1808.——Five guineas were voted to Mr. Barlow for this communication.

It is, perhaps, unnecessary to point out, that a wrench on this principle may be varied in its form and size, so as to be rendered probably more convenient for some particular purposes for which such instruments are required.

I am, sir, your obedient servant,

Portsmouth Dock Yard,
March 1, 1808.

WM. BARLOW.

To C. TAYLOR, M.D. Sec.

Reference to the Engraving of Mr. BARLOW'S *Improved Wrench. See* Plate XIII. Figs. 5, 6, and 7.

This instrument is represented in Plate XIII. Fig. 5 is a perspective view of it; fig. 6 a section of its head; and fig. 7 an external representation of the head. The screw head or nut to be turned is held between two jaws, one of which *a b d e* is forged in the same piece with the handle A A, the other, *f g*, is moveable between two chukes, and fastened to the fixed jaw by the strong screw *i*, which is fixed to the same jaw, passes through the moveable one, as shown in the section fig. 6, and has a nut screwed upon it; the other screw, *h*, is tapped through the moveable jaw, and its point presses upon the bottom of a cavity made in the fixed jaw shown at *m* in the section fig. 6. To make the wrench fit any particular screw head or nut, the nut upon the strong screw *i* must first be loosened, and the screw *h* screwed in or out of the moveable jaw, until the opening *b g* is just the proper width to receive the screw head or nut to be turned by the wrench; the nut of the screw *i* is then to be screwed down, until it presses upon the jaw, and holds it perfectly tight.

LXXXI. *On*

LXXXI. *On the Natural Causes which operate in the Formation of Valleys.* By JOHN CARR, *Esq.*

> "Revolving the circumstances of excavated valleys in my mind, as I have observed them wonderfully distributed over the whole surface of large districts, effecting a descending outlet or drainage to any part: I have been lost in conjecturing any application of mechanical or known principles, that could have directed the almost irresistible forces, which effected this important, and as it were finishing operation on the matter of our globe, but refer the same to Omnipotent Power itself, acting, perhaps, in this instance, without the intervention of the agents whose operations in Nature the light of science enables us in so many instances to trace."
>
> Mr. JOHN FAREY, Philosophical Magazine for April 1809.

TO MR. TILLOCH,—SIR,

THE above "most lame and impotent conclusion" furnishes a very singular instance of the great difference between observing and judging; between the accurate perception of effects, and the more rare and discriminating faculty of drawing from their common agreement and general combination, just and rational deductions of their cause. Phænomena so extensive, combined in such union, and operating so indispensable an office in the wise œconomy of Nature, surely ought to have suggested a more natural and philosophical inference. To me, there are few things more evident than that "the irresistible forces" which have effected the excavation of valleys, are no other than the identical streams which now flow through them; and that by means so natural and obvious, as to excite extreme wonder how so experienced and intelligent an observer, as Mr. Farey unquestionably is, can have surveyed the practical facts, and reflected on the subject, without arriving at the clearest conviction.

Every river which disembogues itself into the ocean is the great drainage trunk of a considerable extent of country, receiving through every part of its course lateral streams, which again receive others, and these others still, in so much that the river itself is frequently the receptacle of hundreds of other streams of various magnitude and extent; and not only the river, but every brook, however remotely connected, has its peculiar range of valleys, which afford it the most easy and direct communication with the stream into which it falls: and all these ranges of valleys are as subservient to, and as intimately connected with, the extensive and general system of drainage of the country, as the streams themselves; and the uniform direction, general connection, and admirable subserviency of the whole, are so palpable, that we are irresistibly led to one of two conclusions,—either that the several ranges of valleys have been purposely and specially formed for the streams which now flow through them, or that the streams themselves have scooped out their own peculiar valleys. The former opinion is too absurd to merit a moment's attention; and the latter has so many direct and positive proofs in its favour, as to yield the most satisfactory conviction to any impartial and competent mind, that will take an actual survey of the spring heads and courses of even the most trivial brooks.

But though there are no natural operations whatever, that from personal inspection more clearly illustrate themselves than this operation of streams in forming their own valleys, it is a subject of considerable difficulty when limited to mere description. The proof circumstances, in all their combinations, are distinct objects of visual inspection, and when spread out beneath the eye exhibit a connected chain of illustrative evidence irresistibly convincing; but the same impressive picture is comparatively faint, and its beautiful unity broken into fragments, like the landscape in the rippling stream, when held up to the "mind's eye" in the closet. Nevertheless there is a bold prominency in the outline of this natural scenery, which even the pen can trace, and I will endeavour to delineate it in a brief and hasty sketch.

The horizontal parallelism of the upper brows of valleys, and of the strata and their identity in the opposite banks, have long ago demonstrated that the strata were formerly continued across, and that the valleys have been formed by the strata being cut through and the missing portions carried away. The truth of this no one will question, who, by actual inspection, has given due attention to the facts upon which it rests.

The source of every stream is always situated on a higher level than that of the country through which the valleys have been cut; and were they all filled up, there would still be a sufficient fall in the country for the streams to flow the same way: and as water, when left to itself, by its fluidity and gravitation constantly seeks the lowest place, we may always be assured that the conrse which the stream has taken is the lowest descent of the country in that direction. With these circumstances in view, let us select any individual stream, and suppose all its valleys to be filled up by replacing the very materials of the strata formerly carried off, thereby restoring the country to its pristine state before the valleys were excavated; and then, by attending to the course of the stream from its source, we shall acquire a clear and correct conception of the manner in which the valleys were originally formed. The old channel being in the lowest fall of the country, the stream will still flow in the same direction, but it will be on and over the newly restored materials, which we have supposed to be replaced; and it will first pass over that portion which has filled up the first valley, until it arrives at what was the lower end, which being now a declivity, it will be precipitated into the hollow below. In that hollow or flat the water will spread itself out into a lake, wider or narrower in its dimensions according to the form and bearing of the ground; and the lake continuing to fill, the water will rise over the level of the materials which filled up the second valley, and running on to where was the lower end, it will again descend the declivity into the hollow below, and will accumulate and spread itself until it again rises over the third valley, and descend again at the lower end; and in this manner it will continue its course, falling down every declivity which it reaches, and accumulating into a lake wherever the nature of the ground obstructs it, until the water teaches over the level of the obstructing rise; and the stream in this stage of its course will consist of a chain of streams, waterfalls and lakes, from its source to the channel of the next stream, or of the sea itself, the grand receptacle of the whole.——Let us now attend to what will take place at the several falls. There, in every instance where the stratum is not an indurated rock, the momentum and action of the descending water will cut a channel, deep and expeditious in proportion to the height of the fall and the yielding nature of the stratum: and as this channel deepens, the unsupported sides will fall in, and the materials be swept away into the lake below. The water will continue this process, but with diminished force, as the inclined plane becomes less steep, until it has again excavated a valley similar to that which we have supposed to be filled up: and this new valley opening directly into the lake above, the lake will, in time, be completely drained off, and the stream will soon work itself out a limited channel in the alluvial materials which have formed the bottom of the lake, and which had been brought down from the detritus of the valley above. After a certain time the whole course of the stream will be changed, in this way, from a succession of streams, waterfalls and lakes, into a succession of valleys and alluvial flats, such as we actually now find existing in the course of almost every stream. If the flat ground a little way beyond any valley be examined, below the vegetable mould, it will be found to consist of sand, gravel, and other alluvial materials precisely similar to the strata in the valley above; and if the valley immediately below be filled up, a lake will forthwith be formed above it, and covering the alluvial materials which had formed the bottom of the former lake:—and these two important facts, capable of the most direct proof in every district where valleys abound, are surely decisive evidence that the original course of the stream did consist of a chain of waterfalls and lakes, and that the falls have worn out the valleys backwards into the lakes above, thereby giving vent to their waters, and leaving the course of the streams, as we now find them, a succession of valleys and alluvial flats.

It may be easily imagined that the valley will be deep and capacious in proportion to the height of the waterfall where the excavation commences, and that the exit of the water in the lake above will increase the fall in the next valley upwards; and it may also be readily conceived, that when the action of the water has worn the inclined plane, down which it descends, to a certain point, all further excavation of the channel will cease, and that the stream may continue to flow for innumerable ages with but trivial alterations in its course. This is the true reason why we do not now see streams forming valleys, the work having been long since accomplished by the channels being reduced to their lowest descending level: and only by the bursting out of a new spring head, in a situation distant from any other stream, could we now practically observe the progression of falls and lakes into valleys and flats, in the manner described. The progress of the stream flowing from such a new spring head would most assuredly establish the truth of every thing I have already stated. For it cannot be doubted that every stream must originally have formed its own channel; and it must be equally obvious, that when first left to find its own way over a great inequality of surface, it must frequently have precipitated down declivities into hollows, out of which it could have no other exit than by swelling into a lake, until the water rose over the level of the lowest ground which bounded the hollow.

In many cases a valley commences immediately at the source of a stream, just opening there and gradually deepening downwards to the lower end, where, questionless, the stream once fell, and where the cutting of the valley commenced; and this form, of being shallow at the upper and deepening down to the lower end, where the fall of the stream first began to act for its formation, is also common to numerous valleys, more especially those near the spring heads; and while it perfectly accords with the action of the stream, it is utterly irreconcileable with any other explanation. Nothing, too, is more usual than the intersection of one valley with another at the confluence of two streams; and in every such instance the angle of intersection of the valleys and streams is acute above and obtuse below, and the two streams invariably meet on precisely the same level. All this would naturally and necessarily result from the streams forming their own valleys and channels, but it is utterly impracticable to assign the most remote probable cause for the same union and unison of effects by any other natural means.

In some cases, indeed, the capacious magnitude of the valley, compared with the diminutive size of the stream, might arrest our belief of so trivial an agent having accomplished so great a work; but those occasional and powerful floods to which every brook is subject, the immense duration of the action, and the yielding nature of the earthy materials removed, are amply sufficient to suppress every doubt and to reconcile every difficulty.

There are multitudes of other practical facts which the actual survey of the course of streams readily supplies, and all of them speaking the same forcible language, of the streams themselves being the only agents that have broken down the opposing obstacles in their course and reduced their channels to the regular gradation slopes down which we now find them flowing.

Those who may be desirous of verifying these observations on the formation of valleys, have only to visit the spring-heads of their neighbouring streams, tracing the channel downwards, and they will find little difficulty in marking the site of every former lake and waterfall, by only supposing the valley which lies before them to be filled up; and the obvious effect will be a lake at the upper and a waterfall at the lower end: and a proper examination of the soil below every valley will discover the very materials formerly brought down when the valley was excavating.

The very intimate connection between waterfalls and lakes, and their disappearance together, by the former effecting breaches into the latter, has been very fully dwelt upon; and accordingly where waterfalls now abound, there ought to be a corresponding abundance of lakes; and this is strikingly corroborated by the fact. Canada is productive of the most numerous and celebrated waterfalls, and there also, *above* these falls, are the most numerous and most extensive lakes on our globe.

Every waterfall that now exists is produced by a stratum of rock crossing the course of the stream, and it is solely owing to the indurated durability of the rock, that we now find a fall where we should otherwise have found a valley. This is so evident, that many of our most celebrated waterfalls are not precipitated from an open height of higher ground, but down into a deep chasm, or rocky valley, formed, undeniably, by the violent action of the water, which has continued, and is still continuing, from the detritus of the rock, to remove the fall higher up the stream: such strictly is the case with the far-famed Falls of Niagara. The surrounding country is nearly on the same level, and the river is propelled over an immense bed of rock, down into a profound valley, which extends for nine miles below the falls, and with every appearance of having been formed by the progressive removal of the fall backwards, in consequence of the gradual waste of the rock by the destructive action of the water.

But there is another point of view in which this magnificent waterfall ought to be considered as eminently illustrative of the subject under consideration. The height of the fall, and what are called the rapids immediately above it, is upwards of two hundred feet; and there is a still further considerable fall in the descent of the river from the lake above.

Now it certainly requires but a small effort of mind to perceive that the vast stratum of imperishable rock which crosses the channel of the river, has alone prevented the St. Lawrence from excavating one of the largest and most capacious valleys on our globe, and that the excavation would have extended upwards into lake Ontario, liberating its waters, and leaving the river to form, for itself, a channel through the central and deepest parts of the exhausted lake.

The retreat of these waters would be productive of another vast fall in the channel from lake Erie, and that fall again excavating a valley upwards, and into the lake, would occasion the exit of its waters; which again would produce a fall and excavation up into lake Huron, the retreat of whose waters would be followed by falls from, and excavations into, lakes Superior and Michigan; and hence this extensive chain of immense lakes would disappear, leaving in its place an equally extensive chain of valleys and alluvial flats, similar, but on a far more gigantic scale, to the thousands of lakes which have disappeared by the same natural process in every country.

I fear, Mr. Editor, I have trespassed much too far on the limits of your truly estimable record of scientific papers; yet I trust the subject is one of superior interest, by bringing us acquainted with the origin of those beautiful excavations which adorn so much of our landscape, and add so much of pleasurable variety to every excursion. They offer, however, still higher claims to our attention from their paramount usefulness in the provident œconomy of Nature, by operating, with such admirable and subservient address, and such a harmonized system of combination, as the universal conduits of all the waters of every country.

I am, sir, your most obedient humble servant,

JOHN CARR.

Princes Street, Manchester,
June 10, 1809.

LXXXII. *Introduction to the Study of Mineralogy.* By *M.* HAUY.

[Concluded from p. 401.]

But the authors of systems of mineralogy, without even excepting chemists, have followed a very different course. They have considered each metal as the base of a particular genus; and in the case in which this metal existed *per se*, in the state of native metal, it would form the first species of the genus; and its combinations with different principles would give the other species. Thus, in the genus of copper, we should have successively, as species, native copper, oxide of copper, sulphuret of copper, carbonate of copper, muriate of copper, &c. In short, metallic substances have characters so striking, that they have been adopted with one accord, as the fixed points around which all the combinations ought to rally of which they form part.

Now, uniformity of method would require, that the same rule which had been followed in the arrangement of metallic substances should also preside over that of substances produced by the union of an earth with an acid; *i. e.* lime, for example, should be considered as the basis of a genus, which should have as its species the combinations of that earth with the carbonic, phosphoric, and fluoric acids. It is evident that all the parts of a clear and distinct distribution should be symmetrical, and that one method cannot be adapted to two different scales; otherwise it would no longer be a method.

But if the natural order should prescribe to us to determine the genera according to the most fixed principle of each compound, nothing should hinder us from generalizing under another view the employment of the acids, by borrowing from these principles a classical character, serving to connect with each other all the substances not metallic of which they form part; and henceforward those substances which bear the name of *salts* would be united in one and the same superior division with others; such as the carbonate of lime, phosphate of lime, sulphate of barytes, &c., which had been ranked among the stones. This intimacy had been already as it were prepared for by the transition of the calcareous sulphate from the class of stones into that of the salts. The characters drawn from the solubility in water and from the taste, so little remarkable in these substances, had almost obliterated the boundary between the two classes; the definition of salts had become vague and equivocal; and it appeared to me that it would be to restore order and precision into the class of bodies which had borne this name, to introduce into it all those which contained an acid joined to an earth or an alkali, and sometimes to both. The collection of all these bodies will therefore form the first class, or that of *acidiferous substances**. I shall exclude the metallic salts, in order to arrange them among the metals, always taking care not to fritter down the genera. This class will be subdivided into three orders, the first of which will comprehend the earthy-acidiferous substances, the second the alkaline-acidiferous substances, and the third the alkalino-earthy-acidiferous substances.

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* The author uses this word uniformly to express those substances into which acids enter as one of their component parts. We shall therefore retain the term in our translation.—Edit.

The second class will be formed of the substances which I call *earthy*, *i. e.* of those which admit no acid among the earths that enter into their composition. I do not think that we are as yet sufficiently acquainted by analysis with the number and proportions of these earths in a part of the substances in question, to be able to subdivide this class into genera. Thus, I shall content myself with presenting the series of the species it contains, taking advantage only (in order to arrange the terms of this series) of analogies or differences which the knowledge we already possess admits of our perceiving among them.

Let us hope that the chemistry of minerals, which since the days of Cronstadt and Bergman has made so great progress, will at length attain a point of perfection which will place this class, and even certain parts of subsequently described classes, upon a level with the first. We have seen for several years discoveries succeed each other rapidly. Klaproth has furnished us with zircon, uranium, titanium, and tellurium. To Vauquelin we are indebted for glucina and chrome.—Analyses made by one person have been verified by others. What may not science gain by this fortunate concurrence?

But if the second class still leaves any thing to desire relative to the regularity of the whole, I flatter myself that I have at least contributed to perfect it in its details, not only by a more exact division of the substances which constitute the species, but also by the care which I have taken to apply this name only to those substances which really deserve it, and to those which have a type susceptible of a precise determination*. Thereby we exclude from the method, and throw into a separate appendix, the argils, marls, and every other similar aggregate composed of fragments borrowed from different species, and consequently possessing mixed characters.

I comprehend under a third class the common name of

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* Thus the beryl and emerald are ranged in one and the same species: the zeolite, on the contrary, is divided into four different species: the strahlstein of the German mineralogists, into two forms very distinct from each other.

combustible substances; the different non-metallic bodies susceptible of combustion, such as the diamond, sulphur, and minerals known by the name of bitumens. Among those substances some have hitherto resisted the attempts made to analyse them; others, treated by distillation or by other means, give out several of the principles which entered into their composition. This difference naturally indicates the subdivision of the class in question into two orders, distinguished from each other by the denominations of *simple* and *compound combustible substances.*

The *metalic substances* still remain, which give the fourth class, subdivided into as many genera as there are metals. Under each of these genera is to be ranked as a species the native metal when it exists; then the metal combined, either with another metal, with oxygen, combustibles, or acids. With respect to the orders which subdivide this class, I have formed them after the example of Bergman, who has borrowed their characters from circumstances which determine their oxidation and reduction, by placing in the first order those which are not oxidizable, but only reducible by heat: in the second, those which are oxidized when heated, and which, when heated more strongly, are reduced: and in the third, those which are oxidizable, but not reducible by heat *.

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* The relations which characterize the divisions and subdivisions of the chemical methods being founded upon the intimate properties and upon the composition of bodies, these methods will at first appear to give way to a certain point to those who employ external characters, and in some measure more accessible characters, in order to establish the classification. I have attempted to supply the place of it, at least relatively to the great divisions, by characters easily ascertained. To conclude: I have not thought that the consideration I have mentioned can balance the advantage of presenting a distribution founded upon the very essence of the substance which it embraces, and at once more symmetrical, more satisfactory to the mind, by giving legitimate order to our ideas. What furnishes an additional motive for supporting this preference is, that, the number of mineralogical species being very inconsiderable, the instant they are once clearly circumscribed, the principal object is fulfilled. For in this case we gain by a little practice such a stock of knowledge, that, when a mineral presents itself for the first time, nothing remains in order to determine it than to decide between two or three species, successively trying the characters which distinguish each of them, until we have removed all doubt.

The choice of a method founded upon the results of analysis naturally led me to adopt, wherever I could, the new chemical nomenclature, so proper in other respects for facilitating the study of science, from the advantages of presenting names truly picturesque, which carry along with them the exact notion of the things they express: but the manner in which my genera were formed, occasions a slight inversion in the denominations, the first word of which ought to express the base of the genus, and the second the specific difference. Consequently, we must substitute, instead of the terms *fluate of lime, sulphate of barytes, sulphate of iron*, &c., the terms *fluated lime, sulphated barytes, sulphated iron*, &c.*

But it is evident that these last denominations produce no real change in the received language; that they require nothing else than memory; and that they present to the mind the same images under the same traces: mineralogy does nothing more here than take the counterproof of the drawing chalked out by the chemist.

I have not dissembled the difficulties which may be presented in the way of my method; but the strongest appeared to me to arise from the state of imperfection in which chemistry still remains with respect to the analysis of some of the minerals. I cannot foresee, for example, the manner in which it might be best to organize and denominate the new genera which future discoveries will produce in the series of earthy substances. I propose the method which seems the least defective in the present state of science. I take advantage of what has been already done, without anticipating what still remains to be done: in short, I stop at the limits prescribed to me by experience, expecting that future labours will extend them.

But it would not have been sufficient to have given to the plan of the system all the regularity and accuracy which practical knowledge requires. I thought myself obliged therefore to extend this plan, by introducing the greatest possible number of species, and by taking advavantage of the recent discoveries which have enriched mineralogy. I here testify my gratitude to those ingenious foreigners to whom I am indebted for some of the greatest rarities in my collection, and particularly to Messrs. Abildgaard, Manthey, Karsten, Neergaard, Esmark, Baron de Moll, Coden, and Hoffman-Bang. To some of these friends I also owe some interesting observations, which have given additional value to their presents. Nothing more strongly confirms what has been so often said of the learned of all countries, namely, *that they form one family*, than this division of riches, which makes the distance between their respective countries disappear, and this communication of light which renders them constantly present with each other.

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* Bergman, who had a very correct mind, and who even assumed fixed principles for the bases of his genera, called *fluorated lime, aerated lime*, &c., what we call *fluated lime, carbonated lime*, &c.

All that I have said concerns the solution of the first of the two problems I have mentioned, and its object is, the classification of substances. Now analysis, which presents data so advantageous for attaining this object, requires operations frequently long and delicate, and on that account alone would become embarrassing, if it was necessary always to have recourse to it, in order to resolve the other problem, *i. e.* in order to recognize the substances.

I now return to the employment of characters which, being more easily ascertained, more convenient and expeditious, may serve as a beacon to the minerals already classified.

To judge of this according to the manner of viewing things generally adopted hitherto with respect to the solution of the problem in question, the simple description of minerals by the helps of their external characters contains all that is sufficient to distinguish them from each other. Nothing has more contributed to establish the reputation of the system in which these characters are employed, than the perfection given to it by Werner. This ingenious mineralogist has presented it under the form of a complete system *, in which every thing in a mineral that is capable of affecting our senses, every thing that is tangible is carefully defined, in which all the different signs by which an attentive observer may recognise it are given by so many expressions, which are afterwards presented separately by themselves, in order to form the picture of each species.

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* Vide *Tabulæ synopticæ terminorum Systematis oryctognostici Werneriani, a Gregorio Wad. Hafniæ*, 1798. See also Berthout and Struve's Principles of Mineralogy, and the plates which accompany Brochant's Mineralogy.

This union of suffrages in favour of the system I have alluded to, the great reputation which it has so justly acquired for its author, would present powerful motives to prevent me from quitting the track he has pointed out. But the plan to which I had referred the order of classification, by only admitting into it species properly so called, susceptible of a rigorous determination, led me, in order to establish their distinct characters, to profit by what they have most constant, most general, and most intimately connected with the constitution of their integrant molecules: and I have given way to the obligation of reconciling with the fixed principles I had adopted, relative to the whole of the science, the method of studying its details.

I shall here briefly mention the reasoning which directed me in the manner of arranging this method. "The picture of a species," I said to myself, "ought to present: 1st, a sum of characters, by the assistance of which an observer can ascertain that a mineral, which he endeavours to class, belongs to this species: 2d, the series of the varieties which subdivide the species.

"Now the specific characters being as the fixed points whence proceeds the knowledge relative to the species, I shall exclude colours, at least when we speak of an earthy or acid substance, as fugitive variable modifications foreign to the type of the species which is the integrant molecule.

"But I shall point out among these characters the specific gravity expressed numerically according to the result of experiments, and note the hardness estimated by the power which the body has to scratch another well-known substance employed to serve as a term of comparison. Nor will I omit the property of double or single refraction, because it belongs to the very basis of substances, although it can be observed but very rarely when these substances are in their natural state. The lustre will be sometimes referred to, not so much with respect to its greater or less intensity, because in this respect it is too much subject to be modified by accidental causes, but relatively to a certain aspect less susceptible of being disguised by the effect of the same causes, and which is, as it were, unctuous in certain minerals, and pearly in others, &c. New characters will, according to circumstances, be associated with the preceding, such as electricity by heat, or phosphorescence by the action of fire.

"I shall endeavour to define precisely the character which is inferred from the mechanical division of a mineral; and instead of confining myself to announce in general if it has taken place in one, two, or three senses, will add the values of the angles which the natural joinings form among each other;—and these joinings being as the first data for attaining the exact determination, either of the primitive form, or of that of the integrant molecule, it will be necessary to indicate these forms, a knowledge of them being important in order to form a just idea of the species.

"Finally, I shall comprehend within the same view, the characters, the verification of which is reserved for agents which, like the acids and caloric, change the nature of a small part of the substance, in order to assist us in becoming acquainted with the whole.

"So much for what concerns the species in general. It will afterwards be requisite to subdivide it; and in order to this, first to consider the varieties relative to the forms, as the most worthy of attention. Each of them will have it's peculiar denomination and definition: and if this form is the produce of a regular crystallization, it will be characterized by an abridged sign * composed of letters and indications of the laws of decrement upon which it depends, which, added to an exact figure, will present the best of all descriptions. I shall add the respective incidences of its faces, determined by theoretical calculation, and in which properly resides the impression borne by a crystal of the species to which it belongs.

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* I shall detail in the generalities the method of writing these signs; and I hope that it will be found simple, and easy of comprehension.

"In short, the modifications relative to colours, transparency, or opacity, will be indicated in their turns, and will form as it were the last shades of the picture."

Thus a specific gravity of about triple that of water, a hardness equal at most to that of such bodies as slightly scratch glass, natural joints parallel to the faces and to the bases of a regular hexahedral prism, the property of dissolving without effervescence in the nitric acid, will easily ascertain that a crystal provided with these properties belongs to the species of phosphated lime; and if it is a regular hexahedral prism terminated by hexahedral pyramids, the faces of which are inclined to each other by about 129°*, this particular character will point out the variety which I denominate *pyramidated phosphated lime*; and the consequence already deduced from the specific character with respect to the nature of the crystal observed, will even then become an evidence, so much the more striking, that this measure of 129° would alone be sufficient for indicating a primitive form of phosphated lime; the analogous inclination being different in the forms of the same genus which belong to other species†. If the same crystal has transparency, if it is of an orange colour, as we find it in Spain, the indication of these accidental circumstances will complete its denomination, and the observer can place it in his collection, with this inscription, *phosphated lime, pyramidal orange-transparent.*

But this will not be a crystal; it will be an irregular mass, in which the geometrical type of the species will have disappeared, and the aspect of it will excite a doubt in the observer, if what he sees be not a coarse carbonated lime, similar to what we call building-stone (*pierre à bâtir*). His doubts will be dispelled, when, having put a small fragment of this mass into the nitric acid, he will obtain a slow and tranquil solution, or at the most accompanied by a slight effervescence; when, having thrown some of it in powder upon lighted charcoal, he will see a fine phosphoric light produced at the same instant. By these traits he will also recognise a phosphated lime; and on examining in detail, upon the picture of this substance, the varieties relative to indeterminable forms, he will learn that the name which he should give to the substance under inspection, will be that of *phosphated, earthy, whitish lime.*

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* More strictly speaking, 129° 13′.

† Quartz, carbonated barytes, phosphated lead.

I shall only add one reflection. We can easily conceive that a pupil, when studying with specimens in his hands, and a system founded upon external characters, can succeed in ascertaining all other specimens which may present themselves to him under the same aspect. But his system having accustomed him to examine objects by the eye and touch, custom has produced an impression upon his mind, which is awakened by their presence, and the cause of which he would be much embarrassed to explain clearly by the help of language. A similar exercise will produce an analogous effect with a person who has at first employed more precise characters; the object which he has considered attentively, after having once determined it, has only occasion to appear: it says enough to his organs, and enables him to dispense with referring to experiment, or to the use of an instrument, unless it has ceased to be familiar to him. But when an unknown object conceals the same intimate composition under an aspect quite different, the student who is accustomed to take as his guide a method purely descriptive, (confined to the circle of bodies, which its author had under his eyes,) may commit a mistake, while another, with the assistance of the first methods, will not be imposed on by a false appearance: and this is a new proof of the preeminence of these characters, which, belonging more closely to the nature of substances, and marking those points which are least likely to escape a common observer, are susceptible of a much greater latitude, and have the double advantage of being applied to our acquired knowledge, and, as it were, to go before those which will subsequently present themselves*.

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* It would be desirable to endeavour to add new physical and chemical characters, simple, and easy to be determined, to those which we already know.

After having presented the picture of the characters, the assemblage of which distinguishes the species, and the series of the varieties which subdivide it, I have added annotations which contain as it were its history. Here will be found the indication of its principal situations in the earth, and that of the substances which most generally accompany it. I afterwards detail the various opinions which have been entertained as to the nature of the mineral which constitutes it; and I thought that it would not be useless to explain, when I had an opportunity, what appeared to have deceived former observers, and how the transition from error to truth was effected. Here again naturally comes the explanation of the phænomena which the mineral is susceptible of presenting, in the event of its enjoying some interesting property. I shall be the more respected by my readers, when I inform them that, in my applications of the subject to the mechanical and healing arts, I have been furnished with valuable assistance by M. Chaptal in the former department, and M. Hallé in the latter.

The point of view under which I have considered mineralogy in this treatise, required that the reader should be prepared for the study of the method, by a detail of the knowledge which was resorted to in forming the plan. I have paid every attention to fulfilling this object in a series of details, in which I develop the principles proper for clearing up the entrance upon the science. I have presented in two ways, the theory of the laws to which the structure of crystals is subjected; the one by simple reasoning aided by figures, which render sensible to the eye the mechanism of this structure; the other in a separate article, by the aid of mathematical analysis, by giving to the results the whole generality which the subject requires *.

I ought

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know. I have found several, which will be pointed out in this treatise; and I am persuaded that we shall succeed, after continued inquiries, to augment the number considerably.

* I am far from thinking that the numerous applications which I have made of this theory to the crystals examined by me, possess all the same degree of exactness. The difficulty of determining, in several of these crystals, the true direction of the natural joinings, the smallness of others, the defects

which,

I ought not to omit how much I am indebted to the intelligence and assiduity of those who have traced the projections relative to crystallography, and to the theories which flow from mineralogical science. The idea of this great work was conceived by M. Brochant, mining engineer, who has begun to realize it. Several other engineers and scientific men have endeavoured to complete what he had begun. M. Tremery, to whom belong, among other things, almost all the projections depending upon calculation, which he perfectly well understands, has carried into their execution the intelligence and accuracy so necessary for enabling the eye easily to catch the respective positions of the different lines, the constructions of which form the whole. Messrs. Cordier, Lefroy, Gallois, Houry, Depuch, Cressac, Ducros, and Hericart, have also given proofs of zeal and talent, in the drawing of figures which relate to the different classes of minerals. Such is the masterly manner in which they have represented, relative to a nucleus which has constantly the same position, the different secondary forms which are so many modifications of it, that we perceive, as at one glance, the relations of these forms, both with each other and with their common nucleus: this is a kind of graphic treatise of the laws to which the structure is subjected.

The School of Mines has offered me another resource of great value, on the subject of the very basis of my work. Placed in an isolated situation for many years, and limited to my own exertions, I occupied myself, in solitude, with arranging the materials for my work; with determining, by observation and theory, all the crystalline forms which I was able to procure; ascending to the causes of the most interesting phænomena presented by minerals; drawing, from the properties of these bodies, characters proper for distinguishing them, and collecting every thing relative to their history, &c. I had even already traced the plan of their methodical

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which, upon those of a more sensible volume, would alter the level of the forces, are so many causes of uncertainty, which ought to influence the solutions of the problems. It is very probable, that observations subsequently made under more favourable circumstances will serve to rectify several of my data, and place the results of calculation on a par with those of Nature.

methodical distribution, which was nearly the same with what I have given here. But in the midst of this complication of inquiries, directed towards so many various objects, there are always some which are attended with doubts, and there are details which either escape us or remain imperfect. I have said enough to convince my readers how advantageous I found it to be placed in the same establishment with Gillet, Lelievre, Lefebvre, Dolomieu, Brogniart, Vauquelin, Coquebert, Tonnellier, from whom I imbibed information and advice. Several important points have been fully and coolly discussed among us; and when the sentiments which flow from a perfect intimacy are freely given in friendly discussions, they produce reflections and observations of great value. The conflict of opinions only paves the way for a better understanding among the disputants, and the cause of true science is uniformly promoted by such discussions.

LXXXIII. *Analysis of the Mécanique Céleste of M.* LA PLACE. *By M.* BIOT.

[Continued from p. 270.]

BOOK SECOND.

AFTER having developed the laws of motion of bodies when actuated by known forces, the author proposes to ascertain what should be the general cause of the celestial motions, in order to reconcile them with actual observations. Setting out therefore with the consideration of the elliptical motion of the planets, and the laws discovered by Kepler, he concludes that the force which attracts the planets and comets is directed towards the centre of the sun, that it is reciprocally as the square of their distances, and that it only differs in different bodies in proportion to these distances. The motion of the satellites around their planets presenting nearly the same phænomena as that of the planets round the sun, the satellites are attracted towards the planets and the sun, by forces reciprocally as the squares of their distances. This law extends to satellites,

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whose orbits have not yet been ascertained to be elliptic; and it follows from this, that, for each system of satellites, the squares of the times of their revolutions are as the cubes of their mean distances from the centre of the planet: the earth having but one satellite, we cannot apply this consideration to it; but the author shows that, if we determine the lunar parallax according to the terrestrial experiments upon gravity, and with the hypothesis that the reciprocal gravitation is as the square of the distance, the result obtained by this way is perfectly conformable to the observations, whence it follows, that the attractive force of the earth is the same as that of all the celestial bodies. These conclusions give rise to several important reflections, from which the author infers this general consequence, that the particles of matter attract each other in the direct ratio of the masses, and the inverse ratio of the square of the distances.

Conformably to this theory, the author establishes the differential equations which determine the motion of a system of bodies subjected to their mutual attraction, and develops the small number of exact integrals which they have hitherto been able to obtain: as observation only makes us acquainted with the relative motions, he gives the formulæ for the motion of a system of bodies subject to the laws of gravitation round a body considered as the centre of their motions, and develops the exact integrals which we know how to deduce from them. In order to go further, recourse must be had to the methods of approximation, and we must profit by the facilities offered for this purpose by the constitution of the system of the world; the author shows that, according to this constitution, the satellites of the planets are moved nearly as if they only obeyed the action of the planet; and the motion of the centre of gravity of a planet, and of its satellites, is very nearly the same as if each of these bodies was collected into its centre. He afterwards proceeds to inquire into the attractive properties of spheroids, and establishes some general propositions on this head, from which it follows that a point placed in the interior of a spheric stratum is equally atttacted from all parts, and that a point without the stratum is attracted by it as if its mass were entirely collected to its centre; properties which also take place with respect to globes formed of concentric layers, of density variable from the centre to the circumference: the author inquires what are the laws of attraction in which these effects subsist; and he proves that, among the infinite number of laws which render the attraction very small at great distances, the law of Nature is the only one in which a spheric stratum attracts a point placed without it, as if it was all collected to its centre: he proves also, that this law is the only one in which the action of the layer upon a point placed within it is nothing: he also makes a second application of the same formulæ, to the case in which the attracting body is a cylinder whose base is a re-entering curve, the length of which is infinite; he demonstrates that, when this curve is a circle, the action of the cylinder upon a point without it, is reciprocally as the distance from its axis to this point; and that, if the attracted point is situated in the interior of a circular cylindric layer of a constant thickness, it is equally attracted from all parts. The formulæ of the motion of a body give rise to some very remarkable conditional equations: the author develops them, and points out their use for verifying the calculations of the theory, and the theory itself of universal gravity; after which he presents the various transformations which it may be most frequently useful to subject the differential equations to, of the motion of any system of bodies animated by their mutual attraction. The bodies which compose the solar system, moving nearly as if they obeyed only the principal force which animates them, and the perturbating forces not being very considerable, the author previously gives as a first approximation, the exact determination of the motion of two bodies which attract each other directly in the ratio of the masses, and inversely as the square of the distances: he explains successively three different methods of integrating differential equations relative to this hypothesis: the second of these methods is founded upon an elegant theorem relative to the integration of differential equations of the first degree, and of any order whatever. The third, which makes the required integrals of one equation only to depend on partial differences, has the advantage of giving the arbitrary quantities in functions of the co-ordinates, and of their first differences, which is frequently useful: the author deduces from it the relations which take place between these arbitrary quantities, and the elements which determine the nature of the conic section and its position in space: finally, he integrates the differential equation which gives the time in a function of the radius vector; and the motion of two bodies is thus determined by three equations, between the eccentric anomaly, the true anomaly, the mean anomaly, and the radius vector of the orbit: these equations being of a nature not capable of being resolved except by approximation, the author details some general theorems upon the reduction of functions into series; and applying these results to the elliptical motion of the planets, he deduces from them the values of the eccentric anomaly, the true anomaly, and of the radius vector, in convergent series of the sines and cosines of the mean anomaly: by referring the motion of the planet to a fixed plane a little inclined to that of the orbit, these series furnish the means of determining by approximation the latitude and longitude of the planet with respect to the fixed plane, as well as the projection of the radius of the orbit upon the same plane. The author explains the theory of motion in a very eccentric ellipsis, and thence deduces the theory of the parabolic motion applicable to comets: he afterwards considers the hyperbolic motion; and then arriving at Kepler's law, according to which the squares of the revolutions of different planets are to each other as the cubes of the transverse axes of their orbits, he shows that this law is not accurately true, and that it only takes place when we neglect the action of the planets upon each other, and upon the sun, and when we consider their masses as infinitely small with respect to that of the sun. He shows the use of these results in determining the ratios of the masses of the planets which have satellites, to the mass of the sun.

After having detailed the theory of elliptic motion, and the method of calculating it by converging series in the two cases of Nature, that of orbits almost circular, and that of orbits

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orbits very much elongated, the author proceeds to determine the elements of these orbits: he shows in the first place how we might deduce them from the circumstances of primitive motion, if these circumstances were known; and it is remarkable that the direction of this motion does not influence the nature of the conic section. These researches produce the discovery of the relation which exists between the transverse axis of the orbit, the chord of the elliptic arc, the sum of the extreme radii vectores, and the time taken to describe this arc.

As the observations do not make known the circumstances attending the primitive motion of the celestial bodies, we cannot determine from this supposition the elements of their orbits: it is necessary for this purpose to compare their respective positions, observed at different epochs, with each other: this is what we may do at all times, with respect to the planets, which we may observe without interruption; but it is not the case with comets, which are only visible to us in that part of their orbit which is nearest to the sun: it is important, therefore, to be able to determine the elements of the orbit of a comet from the circumstances attending its appearance. In order to attain this, the author in the first place gives converging formulæ, which make known for a given time, and according to any number of adjacent observations, the geocentric longitude and latitude of the comet, as well as their first and second differences divided by the corresponding powers of the element of the time: he shows that, by supposing these quantities to be known for a given time in a system of bodies subjected to their mutual attraction, we may easily, and without the assistance of integration, deduce therefrom the elements of the orbits.

After having detailed these methods to the utmost extent that is necessary, and given them all the perfection of which they are susceptible; attending also in a very simple manner to the eccentricity of the terrestrial orbit, the author applies them to the case of Nature, in which the orbits of comets are ellipses greatly elongated, which are sensibly confounded with the parabola towards the perihelion, which admits of our considering their transverse axes as infinite: this circumstance,

cumstance, which makes known *à priori* one of the elements of the orbit, introducing a new equation, it follows from it that the determination of parabolic orbits of comets conduces to more equations than unknown quantities, which gives room for various methods of calculating them. After having ascertained that method which ought to give the greatest precision, the author enters upon the subject at full length, and divides it into two pars: in the first he determines nearly the perihelion distance of the comet, and the time of its passage by the perihelion: in the second he gives the method of correcting these two elements by means of three observations taken at a distance from each other, and he deduces all the others from them. There exists a peculiar case in which the orbit of the comet may be rigorously determined: it is that case wherein the comet has been observed in its two nodes: after having examined it, the author gives the corrections necessary to be made in the elements calculated in the parabola to obtain the corresponding elements in the ellipsis: these inquiries, applied to comets, furnish the method of determining nearly the duration of their revolutions, when we have a great number of very exact observations, both before and after the passage by the perihelion. The method explained has the double advantage of correcting, by the number of observations, the influence of their errors, and of giving the elements by a rigorous analysis, simply by making the approximations to fall upon those data which are given by observation.

[To be continued.]

LXXXIV. *Reply to Mr.* Barlow's *Article on Floating Bodies. By* G. Orr, *Esq., of Buckingham Place, Fitzroy Square.*

To Mr. Tilloch,—Sir,

In sending for your Magazine of March last a short essay on the subject of barges, timber, &c., floating down rivers, streams, or currents, it was not my intention to court a paper war. But Mr. Barlow* having asserted that I did not understand the subject, I beg to be allowed to trespass again on your indulgence.

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* Philosophical Magazine for April 1809, p. 300.

I am persuaded that no person who has investigated the subject, will deny that all water in motion descends an inclined plane, and that bodies floating in it are actually descending an inclined plane also, being influenced by two causes in their progress; first, by the motion communicated to them by the fluid in which they float; and secondly, by their own weight arising from the inherent property of gravity, which, whether the body be more or less specifically heavy, is immutable, and peculiar to all matter.—*In vacuo*, as every one knows, a feather and a piece of gold will descend with equal velocity, and pass through equal spaces in equal times; but in air or water, the progress of bodies specifically different, will vary according to their specific gravity; the lighter body, possessing less power to overcome opposition, must of course be slower in its progress.—If the wood and metal balls, which I mentioned, were let fall *in vacuo*, or if they were mathematically polished, and passed down an inclined plane so polished and placed in *vacuo*, they would both descend with the same velocity; but in open air, and on a rough surface, the heaviest body being possessed of greater power, arising from a greater quantity of matter, viz. from greater specific gravity, it will have the greater power in overcoming opposition, and will pass on with the more rapid motion; but still in both bodies the principle of gravity is the same.

Mr. B. says that I am mistaken in making a comparison between balls of different weights rolling down an inclined plane, and barges or beams of different weights floating down a running stream: that is, as I understand him, he does not like the comparison, and he says the balls move through a *medium* perfectly at rest, but the barges, &c., through a *medium* in motion.—I beg leave to observe to Mr. B., that the air or atmosphere is never perfectly at rest, except when all its particles are *in equilibrio*, which is seldom the case, and never generally so. Balls may meet currents of air, or may overtake air moving slower than themselves: in either case, their motion must be retarded more or less, the same as beams floating in running water may be retarded by encountering irregular or slower motions in water.

Mr. B. illustrates his opinion by supposing a beam of timber loaded at one end, placed in running water, and *moving parallel to itself*, or, in other words, having an equal velocity at both ends. But this is absolutely impossible in the nature of things. The beam thus loaded cannot preserve its parallelism for an instant. It must obey the laws of gravity, and instantaneously begin to change its situation, the heavy end getting foremost. It would be just as impossible that such a loaded beam could preserve its parallelism, as that a ruler loaded at one end, and placed parallel on an inclined plane, should rest in that position, or that a cone should keep in such a situation without force.—Mr. B. says again, "If a beam should meet any resistance, that end which is heaviest will oppose it with the greatest effect." Certainly, because it contains the greatest quantity of matter: But will not the heavier end always go foremost without any resistance? In considering this subject, allowance must be made for currents, eddies, &c.; but to understand it the more clearly, it would be best to consider the floating bodies as passing down a regular stream.

Mr. B. seems to me to confound weight, which is only the result of gravity, and of a greater quantity of matter, with gravity itself; for though one end of a beam be heavier than the other, and the beam consequently will float with the heavier end foremost, yet the inherent and inseparable property of gravity is the same in both ends of the beam.

I am, sir, your obedient servant,

G. Orr.

P. S. I do conceive that all bodies floating with the stream, and which have a heavy and light end, will become depressed at their heavier end, and be borne parallel to the plane on which the water moves,—in all such cases adapting themselves to the inclination of the plane or bed on which the water runs.

LXXXV. *The*

LXXXV. *The Bakerian Lecture. An Account of some new analytical Researches on the Nature of certain Bodies, particularly the Alkalies, Phosphorus, Sulphur, Carbonaceous Matter, and the Acids hitherto undecomposed; with some general Observations on Chemical Theory.* By HUMPHRY DAVY, *Esq., Sec. R.S., F.R.S. Edin., and M.R.I.A.*[*]

I. *Introduction.*

In the following pages, I shall do myself the honour of laying before the Royal Society, an account of the results of the different experiments, made with the hopes of extending our knowledge of the principles of bodies by the new powers and methods arising from the applications of electricity to chemistry, some of which have been long in progress, and others of which have been instituted since their last session.

The objects which have principally occupied my attention, are the elementary matter of ammonia, the nature of phosphorus, sulphur, charcoal, and the diamond, and the constituents of the boracic, fluoric, and muriatic acids.

Amongst the numerous processes of decomposition, which I have attempted, many have been successful; and from those which have failed, some new phænomena have usually resulted which may possibly serve as guides in future inquiries. On this account, I shall keep back no part of the investigation, and I shall trust to the candour of the Society for an excuse for its imperfection.

The more approaches are made in chemical inquiries towards the refined analysis of bodies, the greater are the obstacles which present themselves, and the less perfect the results.

All the difficulties which occur in analysing a body, are direct proofs of the energy of attraction of its constituent parts. In the play of affinities with respect to secondary compounds even, it rarely occurs that any perfectly pure or unmixed substance is obtained; and the principle applies still more strongly to primary combinations.

The first methods of experimenting on new objects likewise

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[*] From Philosophical Transactions for 1809. Part I.

wise are necessarily imperfect; novel instruments are demanded, the use of which is only gradually acquired, and a number of experiments of the same kind must be made, before one is obtained from which correct data for conclusions can be drawn.

II. *Experiments on the Action of Potassium on Ammonia, and Observations on the Nature of these two Bodies.*

In the Bakerian lecture, which I had the honour of reading before the Society, November 19, 1807, I mentioned that, in heating potassium strongly in ammonia, I found that there was a considerable increase of volume of the gas, that hydrogen and nitrogen were produced, and that the potassium appeared to be oxidated; but this experiment, as I had not been able to examine the residuum with accuracy, I did not publish. I stated it as an evidence, which I intended to pursue more fully, of the existence of oxygen in ammonia.

In a paper read before the Royal Society last June, which they have done me the honour of printing, I have given an account of various experiments on the amalgam from ammonia, discovered by Messrs. Berzelius and Pontin, and in a note attached to this communication I ventured to controvert an opinion of MM. Gay Lussac and Thenard, with respect to the agency of potassium and ammonia, even on their own statement of facts, as detailed in the Moniteur for May 27, 1808.

The general obscurity belonging to these refined objects of research, their importance and connection with the whole of chemical theory, have induced me since that time to apply to them no inconsiderable degree of labour and attention; and the results of my inquiries will, I trust, be found not only to confirm my former conclusions; but likewise to offer some novel views.

In the first of these series of operations on the action of potassium on ammonia, I used retorts of the green glass; I then, suspecting oxygen might be derived from the metallic oxides in the green glass, employed retorts of plate glass; and last of all, I fastened the potassium upon trays of platina, or iron, which were introduced into the glass retorts furnished

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furnished with stop cocks. These retorts were exhausted by an excellent air pump, they were filled with hydrogen, exhausted a second time, and then filled with ammonia from an appropriate mercurial gas holder*. In this way the gas was operated upon in a high degree of purity, which was always ascertained; and all the operations performed out of the contact of mercury, water, or any substances that could interfere with the results.

I at first employed potassium procured by electricity; but I soon substituted for it the metal obtained by the action of ignited iron upon potash, in the happy method discovered by MM. Gay Lussac and Thenard, finding that it gave the same results, and could be obtained of an uniform quality †, and in infinitely larger quantities, and with much less labour and expense.

When ammonia is brought in contact with about twice its weight of potassium at common temperatures, the metal loses its lustre and becomes white, there is a slight diminution in the volume of the gas; but no other effects are produced. The white crust examined proves to be potash, and the ammonia is found to contain a small quantity of hydrogen, usually not more than equal in volume to the metal. On heating the potassium in the gas, by means of a spirit lamp applied to the bottom of the retort, the colour of the crust is seen to change from white to a bright azure, and this gradually passes through shades of bright blue and green into dark olive. The crust and the metal then fuse together; there is a considerable effervescence, and the crust passing off to the sides, suffers the brilliant surface of the potassium to appear. When the potassium is cooled in this state, it is again covered with the white crust. By heating a second time, it swells considerably, becomes porous, and appears crystallized, and of a beautiful azure tint; the same series of phænomena, as those before described, occur in a continuation of the process, and it is finally entirely converted into the dark olive-coloured substance.

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* A representation of the instruments will be given in the next Number of our Magazine.

† When the potash used for procuring potassium in this operation was very pure, and the iron turnings likewise very pure and clean, and the whole apparatus free from any foreign matters, the metal produced differed very little, in its properties, from that obtained by the Voltaic battery. Its lustre, ductility, and inflammability were similar. Its point of fusion and specific gravity were, however, a little higher, it requiring nearly 130° of Fahrenheit to render it perfectly fluid, and being to water as 7960 to 10000, at 60° Fahrenheit. This I am inclined to attribute to its containing a minute proportion of iron.

In this operation, as has been stated by MM. Gay Lussac and Thenard, a gas which gives the same diminution by detonation with oxygen as hydrogen is evolved, and ammonia disappears.

The proportion of the ammonia which loses its elastic form, as I have found by numerous trials, varies according as the gas employed contains more or less moisture.

Thus eight grains of potassium, during its conversion into the olive-coloured substance, in ammonia saturated with water at 63° Fahrenheit, and under a pressure equal to that of 29·8 inches of mercury, had caused the disappearance of twelve cubical inches and a half of ammonia; but the same quantity of metal acted upon under similar circumstances, except that the ammonia had been deprived of as much moisture as possible by exposure for two days to potash that had been ignited, occasioned a disappearance of sixteen cubical inches of the volatile alkali.

Whatever be the degree of moisture of the gas, the quantities of inflammable gas generated have always appeared to me to be equal for equal quantities of metal. MM. Gay Lussac and Thenard are said to have stated, that the proportions in their experiment were the same as would have resulted from the action of water upon potassium. In my trials, they have been rather less. Thus, in an experiment conducted with every possible attention to accuracy of manipulation, eight grains of potassium generated, by their operation upon water, eight cubical inches and a half of hydrogen gas: and eight grains from the same mass, by their action upon ammonia, produced eight cubical inches and one-eighth of inflammable gas. This difference is inconsiderable; yet I have always found it to exist, even in cases where the ammonia has been in great excess, and every part of the metal apparently converted into the olive-coloured substance.

No other account of the experiments of MM. Gay Lussac and Thenard has, I believe, as yet been received in this country, except that in the Moniteur already referred to; and in this no mention is made of the properties of the substance produced by the action of ammonia on potassium. Having examined them minutely and found them curious, I shall generally describe them.

1. It is crystallized, and presents irregular facets, which are extremely dark, and in colour and lustre not unlike the protoxide of iron; it is opaque when examined in large masses, but is semi-transparent in thin films, and appears of a bright brown colour by transmitted light.

2. It is fusible at a heat a little above that of boiling water, and if heated much higher, emits globules of gas.

3. It appears to be considerably heavier than water, for it sinks rapidly in oil of sassafras.

4. It is a non-conductor of electricity.

5. When it is melted in oxygen gas, it burns with great vividness, emitting bright sparks. Oxygen is absorbed, nitrogen is emitted, and potash, which from its great fusibility seems to contain water, is formed.

6. When brought in contact with water, it acts upon it with much energy, produces heat, and often inflammation, and evolves ammonia. When thrown upon water, it disappears with a hissing noise, and globules from it often move in a state of ignition upon the surface of the water. It rapidly effervesces and deliquesces in air, but can be preserved under naphtha, in which, however, it softens slowly, and seems partially to dissolve. When it is plunged under water filling an inverted jar, by means of a proper tube, it disappears instantly with effervescence, and the non-absorbable elastic fluid liberated is found to be hydrogen gas.

By far the greatest part of the ponderable matter of the ammonia, that disappears in the experiment of its action upon potassium, evidently exists in the dark fusible product. On weighing a tray containing six grains of potassium, before and after the process, the volatile alkali employed having been very dry, I found that it had increased more than two grains; the rapidity with which the product acts upon moisture, prevented me from determining the point with great minuteness; but I doubt not, that the weight of the olive-coloured substance and of the hydrogen disengaged precisely equals the weight of the potassium, and ammonia consumed.

MM. Gay Lussac and Thenard* are said to have procured from the fusible substance, by the application of a strong heat, two fifths of the quantity of ammonia that had disappeared in their first process, and a quantity of hydrogen and nitrogen in the proportions in which they exist in ammonia, equal to one fifth more.

My results have been very different, and the reasons will, I trust, be immediately obvious.

When the retort containing the fusible substance is exhausted, filled with hydrogen and exhausted a second time, and heat gradually applied, the substance soon fuses, effervesces, and, as the heat increases, gives off a considerable quantity of elastic fluid, and becomes at length, when the temperature approaches nearly to dull redness, a dark gray solid, which, by a continuance of this degree of heat, does not undergo any alteration.

In an experiment, in which eight grains of potassium had absorbed sixteen cubical inches of well dried ammonia in a glass retort, the fusible substance gave off twelve cubical inches and half of gas, by being heated nearly to redness, and this gas analysed, was found to consist of three quarters of a cubical inch of ammonia, and the remainder of elastic fluids, which, when mixed with oxygen gas in the proportion of $6\frac{1}{2}$ to 6, and acted upon by the electric spark, diminished to $5\frac{1}{2}$. The temperature of the atmosphere, in this process, was 57° Fahrenheit, and the pressure equalled that of 30·1 inches of mercury.

In a similar experiment, in which the platina tray containing the fusible substance was heated in a polished iron tube,

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* No notice is taken of the apparatus used by MM. Gay Lussac and Thenard in the Moniteur; but, from the tenour of the details, it seems that they must have operated in glass vessels in the way heretofore adopted over mercury.

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filled with hydrogen gas, and connected with a pneumatic apparatus containing very dry mercury, the quantity of elastic fluid given off, all the corrections being made, equalled thirteen cubical inches and three quarters, and of these a cubical inch was ammonia; and the residual gas, and the gas introduced into the tube being accounted for, it appeared that the elastic fluid generated, destructible by detonation with oxygen, was to the indestructible elastic fluid, as 2·5 to 1.

In this process, the heat applied approached to the dull red heat. The mercury, in the thermometer, stood at 62° Fahrenheit, and that in the barometer at 30·3 inches.

In various experiments on different quantities of the fusible substance, in some of which the heat was applied to the tray in the green glass retort, and in others, after it had been introduced into the iron tube; and in which the temperature was sometimes raised slowly and sometimes quickly, the comparative results were so near these that I have detailed, as to render any statement of them superfluous.

A little more ammonia, and rather a larger proportion of inflammable gas*, were in all instances evolved when the iron tube was used, which I am inclined to attribute to the following circumstances. When the tray was brought through the atmosphere to be introduced into the iron tube, the fusible substance absorbed a small quantity of moisture from the air, which is connected with the production of ammonia. And in the process of heating in the retort, the green glass was blackened, and I found that it contained a very small quantity of the oxides of lead and iron, which must have caused the disappearance of a small quantity of hydrogen.

MM. Gay Lussac and Thenard, it appears from the statement, had brought the fusible substance in contact with mercury, which must have given to it some moisture; and whenever this is the case, it furnishes by heat variable quantities of ammonia. In one instance, in which I heated the fusible substance from nine grains of potassium, in a

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* The average of six experiments made in a tube of iron, is 2·4 of inflammable gas to 1 of uninflammable. The average of three made in green glass retorts, is 2·3 to 1.

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retort that had been filled with mercury in its common state of dryness, I obtained seven cubical inches of ammonia as the first product; and in another experiment which had been made with eight grains, and in which moisture was purposely introduced, I obtained nearly nine cubical inches of ammonia, and only four of the mixed gases.

I am inclined to believe, that if moisture could be introduced only in the proper proportion, the quantity of ammonia generated, would be exactly equal to that which disappeared in the first process.

This idea is confirmed by the trials which I have made, by heating the fusible substance with potash, containing its water of crystallization, and muriate of lime partially dried*.

In both these cases, ammonia was generated with great rapidity, and no other gas, but a minute quantity of inflammable gas, evolved, which was condensed by detonation with oxygen with the same phænomena as pure hydrogen.

In one instance, in which thirteen cubical inches of ammonia had disappeared, I obtained nearly eleven and three quarters by the agency of the water of the potash; the quantity of inflammable gas generated, was less than four tenths of a cubical inch.

In another, in which fourteen cubical inches had been absorbed, I procured by the operation of the moisture of muriate of lime, nearly eleven cubical inches of volatile alkali, and half a cubical inch of inflammable gas; and the differences, there is every reason to believe, were owing to an excess of water in the salts, by which some of the gas was absorbed.

Whenever, in experiments on the fusible substance, it has been procured from ammonia saturated with moisture, I have always found that more ammonia is generated from it by mere heat; and the general tenour of the experiments inclines me to believe, that the small quantity, produced in experiments

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* If water, in its common form, is brought in contact with the fusible substance, it is impossible to regulate the quantity, so as to gain conclusive results, and a very slight excess of water causes the disappearance of a very large quantity of the ammonia generated. In potash and muriate of lime, in certain states of dryness, the water is too strongly attracted by the saline matter to be given off, except for the purpose of generating the ammonia.

ments performed in vacuo, is owing to the small quantity of moisture furnished by the hydrogen gas introduced, and that the fusible substance, heated out of the presence of moisture, is incapable of producing volatile alkali.

MM. Gay Lussac and Thenard, it is stated, after having obtained three fifths of the ammonia or its elements that had disappeared in their experiment, by heating the product, procured the remaining two fifths, by adding water to the residuum, which after this operation was found to be potash. No notice is taken of the properties of this residuum, which, as the details seem to relate to a single experiment, probably was not examined; nor, as moisture was present at the beginning of their operations, could any accurate knowledge of its nature have been gained.

I have made the residuum of the fusible substance after it has been exposed to a dull red heat, out of the contact of moisture, an object of particular study, and I shall detail its general properties.

It was examined under naphtha, as it is instantly destroyed by the contact of air.

1. Its colour is black, and its lustre not much inferior to that of plumbago.

2. It is opaque even in the thinnest films.

3. It is very brittle, and affords a deep gray powder.

4. It is a conductor of electricity.

5. It does not fuse at a low red heat, and when raised to this temperature, in contact with plate glass, it blackens the glass, and a grayish sublimate rises from it, which likewise blackens the glass.

6. When exposed to air at common temperatures, it usually takes fire immediately, and burns with a deep red light.

7. When it is acted upon by water, it heats, effervesces most violently, and evolves volatile alkali, leaving behind nothing but potash. When the process is conducted under water, a little inflammable gas is found to be generated. A residuum of eight grains giving in all cases about $\frac{20}{100}$ of a cubical inch.

8. It has no action upon quicksilver.

9. It combines with sulphur and phosphorus by heat,

without any vividness of effect, and the compounds are highly inflammable, and emit ammonia, and the one phosphuretted and the other sulphuretted hydrogen gas, by the action of water.

[To be continued.]

LXXXVI. *On the Agency of Electricity on Animal Secretions.* *By* Wm. H. Wollaston, *M.D., Sec. R.S.*

At the time when Mr. Davy first communicated to me his important experiments on the separation and transfer of chemical agents by means of the Voltaic apparatus, which was in the autumn of 1806, I was forcibly struck with the probability that animal secretions were affected by the agency of a similar electric power; since the existence of this power in some animals was fully proved by the phænomena of the Torpedo, and of the Gymnotus Electricus; and since the universal prevalence of similar powers of lower intensity in other animals was rendered highly probable by the extreme suddenness with which the nervous influence is communicated from one part of the living system to another.

And though the separation of chemical agents, as well as their transfer to a distance, and their transition through solids, and through fluids which might be expected to oppose their progress, had not then been effected but by powerful batteries; yet it appeared highly probable that the weakest electric energies might be capable of producing the same effects, though more slowly in proportion to the weakness of the powers employed.

I accordingly at that time made an experiment for elucidating this hypothesis, and communicated it to Mr. Davy and to others of my friends. But though it was conclusive with regard to the sufficiency of very feeble powers, it did not appear deserving of publication, until I could adduce some evidence of the actual employment of such means in the animal œconmy.

As I am not accustomed to making experiments on living animals, I had deferred pursuing the application of my theory, until it was again brought back to my mind by finding that the same thought had occurred to Dr. Young. And as it has already been printed some months in the Syllabus of his Course of Medical Lectures, I had for the present relinquished all thoughts of recording conjectures, which, if not well founded, might retard the progress of science.

But since some experiments relating to the same inquiry are now about to be published by Mr. Home, it may perhaps be of use to add my experiment to the general stock of information, although I have not myself improved upon it by any further consideration, and am not yet enabled to confirm the hypothesis, which it appeared to support, by any new arguments.

The experiment was conducted as follows:

I took a piece of glass tube about three quarters of an inch in diameter and nearly two inches long, open at both ends, and covered one of them with a piece of clean bladder. Into this little vessel I poured some water in which I had dissolved $\frac{1}{240}$ of its weight of salt; and after placing it upon a shilling with the bladder slightly moistened externally, I bent it a wire of zinc so, that while one extremity rested on the shilling the other might be immersed about an inch in the water. By successive examination of the external surface of the bladder, I found that even this feeble power occasioned soda to be separated from the water, and to transude through the substance of the bladder. The presence of alkali was discernible by the application of reddened litmus-paper after two or three minutes, and was generally manifest even by the test of turmeric before five minutes had expired.

The efficacy of powers so feeble as are here called into action, tends to confirm the conjecture that similar agents may be instrumental in effecting the various animal secretions, which have not yet been otherwise explained. The qualities of each secreted fluid may hereafter instruct us as to the species of electricity that prevails in each organ of the body.

For instance, the general redundance of acid in urine,

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though secreted from blood that is known to be alkaline, appears to indicate in the kidneys a state of positive electricity; and since the proportion of alkali in bile seems to be greater than is contained in the blood of the same animal, it is not improbable that the secreting vessels of the liver may be comparatively negative.

With such views of the vital functions it becomes an interesting subject of inquiry, what other organs may also be considered as permanently different in their state of electricity, and what others may possibly be subject to temporary states of opposite electric energies, and may, by means of such relation, produce the most powerful effects in the animal œconomy.

LXXXVII. *Report of Surgical Cases in the City and Finsbury Dispensaries for October, November, and December,* 1808. *With the Dissection of a singular Fœtus.* By JOHN TAUNTON, *Esq.*

In October, November, and December, there were admitted on the books of the City and Finsbury Dispensaries 738 surgical patients.

Cured or relieved —	321
Died — —	7
Irregular — —	3
Under treatment —	407
	738

During the year 1808, there have been admitted on the books of the City Dispensary 3105 patients.

Cured or relieved —	3069
Discharged for irregularity	5
Died — —	31
	3105

The expense of the City Dispensary for the year 1808, including every item of disbursement, amounts only to 575*l.* 9*s.* 1*d.*; a sum comparatively small to the benefits derived by the lower order of society, by persons incapable of providing either attendance or medicine otherwise. The few deaths are perhaps the strongest proof of the advantage of medical establishments of this kind, where the patients are treated in the midst of their families, and where they enjoy the kind and affectionate offices of the healthful.

"To contrast dispensaries with hospitals might seem invidious; but the dispensary has this peculiar advantage, that it retains the unhappy sufferers in the bosom of their families:—Judge of this, you who have felt the miseries of parting with those who are nearest and dearest to you, and who have experienced the anxious cares and sympathies of an affectionate husband or wife, of a tender parent or child, in the day of distress!—Judge of it also, ye who are acquainted with the nature of the animal œconomy;—with the influence of the mental faculties and passions over the body;—with the injurious effects of depressing, and the beneficial and important consequences of soothing, passions in the alleviation or cure of disease*."

Mrs. —— has had several children, none of whom have lived:—she was taken in labour at three P. M.: nothing remarkable occurred previous to the expulsion of the fœtus, except that there was a larger quantity of the liquor amnii discharged soon after the labour commenced than is usual; the pains returned at irregular periods, from 15 to 30 minutes, for 36 hours, without producing any descent of the head; the pains thus became stronger, and returned at shorter intervals, so as to expel the head in three quarters of an hour; the shoulders and body followed soon by a few more pains.

The pulsation had ceased in the funis, but soon returned; and the infant showed signs of life sufficient to encourage the hope of respiration being established, the whole surface of the body being changed from a livid to a florid hue.

The inspirations became more frequent, and were attended with convulsive twitchings; the motion of the heart was very evident, but there was not any pulse at the wrist distinguishable; the vital functions gradually diminished, and ceased in three hours after birth.

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* Introduction to Dispensary Rules and Regulations.

Dissection.—A small quantity of a yellowish fluid was found in each cavity of the thorax; the mediastinum was attached only to the right side of the sternum; the heart was placed on the right side, and no part of that organ reached beyond the middle of the sternum; the right lung *very* small, but divided into three lobes.

The left cavity of the thorax contained the small and part of the large intestines, which had protruded through an opening in the diaphragm of a sufficient size to admit of their being essily retracted; the left lung was also *very* small, but divided into two lobes.

The cavities of the heart were natural; the foramen ovale of its usual appearance; the canalis arteriosus was large; the pulmonary vessels were small, particularly on the left side, corresponding to the diminished state of the lung.

The vessels from the curve of the aorta and intercostals were distributed as usual; the phrenic, coelic, emulgent, spermatic, and umbilical vessels were natural; the trunk of the superior mesenteric artery was continued through the opening of the diaphragm to supply the abdominal viscera that had protruded into the thorax; the inferior mesenteric and the vessels going to the lower extremities were distributed in the usual manner.

The liver was large; the gall-bladder and vessels were perfect; the spleen, pancreas and stomach were well formed; the duodenum ascended in its course across the spine to the opening in the diaphragm; the jejunum ilium, cæcum, ascending and transverse colon, were situated in the thorax, where they appear to have been formed, as from the attachment of the mediastinum and size of the lung there could not have been any viscera to have occupied the left cavity of the chest.

The descending colon passed through the same opening into the abdomen over the left kidney, and formed the sigmoid flexion on the brim of the pelvis, and terminated in the rectum.

The superior part of the left kidney was opposed to the opening in the diaphragm, and could be seen from the thorax on raising the intestines.

The child was much above the standard size, but the external parts of the body were well formed.

Death ensued in consequence of the diminished capacity of the lungs, their organs not being of sufficient size to admit air in quantity sufficient for the continuance of respiration.

The preparation, and drawings made from the same, are preserved in my museum, and may be seen by any person who is desirous.

JOHN TAUNTON,
Surgeon to the City and Finsbury Dispensaries, and City Truss Society, Lecturer on Anatomy, Surgery, Physiology, &c.

Greville street, Hatton Garden,
May 18, 1809.

LXXXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

THE First Part of this Society's Transactions has just been published. The following are its contents:

1. The Croonian Lecture. On the Functions of the Heart and Arteries. By Thomas Young, M.D., For. Sec. R.S.—2. An Account of some Experiments, performed with a View to ascertain the most advantageous Method of constructing a Voltaic Apparatus, for the Purposes of Chemical Research. By John George Children, Esq., F.R.S.—3. The Bakerian Lecture. An Account of some new analytical Researches on the Nature of certain Bodies, particularly the Alkalies, Phosphorus, Sulphur, Carbonaceous Matter, and the Acids hitherto undecompounded; with some general Observations on Chemical Theory. By Humphry Davy, Esq., Sec. R.S., F.R.S. Ed. and M.R.I.A.—4. An Account of a Method of dividing Astronomical and other Instruments, by ocular Inspection; in which the usual Tools for graduating are not employed; the whole Operation being so contrived, that no Error can occur but what is chargeable to Vision, when assisted by the best optical Means of viewing and measuring minute Quantities. By Mr. Edward Troughton. Communicated by the Astronomer Royal.—5. A Letter on a Canal in the Medulla Spinalis of some Quadrupeds. In a

Letter from Mr. William Sewell to Everard Home, Esq., F.R.S.—6. A numerical Table of elective Attractions; with Remarks on the Sequences of double Decompositions. By Thomas Young, M.D. For. Sec. R.S.—7. Account of the Dissection of a Human Fœtus, in which the Circulation of the Blood was carried on without a Heart. By Mr. B. C. Brodie. Communicated by Everard Home, Esq., F.R.S.—8. On the Origin and Formation of Roots. In a Letter from T. A. Knight, Esq., F.R.S., to the Right Hon. Sir Joseph Banks, Bart., K. B. P.R.S.—9. On the Nature of the intervertebral Substance in Fish and Quadrupeds. By Everard Home, Esq , F.R.S.

June 1.—The president in the chair. The conclusion of Dr. Henry's paper on the decomposition of ammonia was read. The result of the author's present experiments led him to perceive some errors in those of his preceding paper, and to conclude that the oxygen which he had disengaged from ammonia by electrization was derived from other bodies, and not from the ammonia; consequently that ammonia should not yet be considered as an oxide.

Mr. Davy read some remarks on Dr. Henry's experiments, which tended to prove that the composition of ammonia cannot be ascertained till the nature of nitrogen is determined. Dr. H. thought the proportion of hydrogen in ammonia, as determined by Mr. Davy, rather low, and estimated it at 72 hydrogen and 28 nitrogen, instead of 74 hydrogen and 26 nitrogen; but Mr. D. having repeated his experiments, found them very nearly correct, and took 73—27 as the truth.

An ingenious paper by the Rev. Mr. Lax, professor of astronomy at Cambridge, was read, on the means of graduating and correcting mathematical instruments. The author uses Carey's semi-circle of a foot diameter, corrects it by microscopes and observations, and adjusts it so as to counteract the expansion and contraction by change of temperature.

June 8.—Dr. Wollaston read a paper proving the identity of columbium and tantalium, the former discovered by Mr. Hatchett, the latter by the Swedish chemist Ekeberg. Dr. W. pro-

W. procured some grains of the original specimens from the British Museum, and from Mr. Hatchett; and notwithstanding the smallness of the quantity, he succeeded in proving them to be radically the same metal.

Dr. Wollaston also read another ingenious paper discovering a method of constructing a gonyometer for measuring the angles of crystals by means of reflection, with microscopes, which enable the observer to ascertain accurately the angles of crystals, whether rough or smooth. Dr. W. applied this useful instrument, of which a drawing was exhibited, to crystals of carbonate of lime.

A mathematical paper by Mr. Ivory was laid before the Society.

June 15.—A paper by Sir James Earle was read, describing a stone in the bladder, which occupied its whole contents, and weighed 44 ounces.

The Society for improving animal chemistry furnished a paper by Mr. Brandé, detailing the results of a series of experiments on animal mucus and albumen exposed to galvanic electricity.

A paper by Dr. Pearson, on expectorated matter, was read. It appears from the Doctor's experiments, that the different kinds of expectorated matter differ rather in the proportion of the ingredients than in kind. They all consist of albuminous matter, water, and the two principal ingredients are muriate of soda and potash neutralized by animal oxide, if not by a destructible acid, besides a small proportion of phosphate of lime, ammonia, carbonate of lime, and probably phosphate of magnesia and siliceous earth. The Doctor announces that potash neutralized by animal matter is contained in the blood, and in most or all of the secreted and excreted fluids, namely, in dropsical water, pus, both that secreted without breach of surface as well as that of abscesses, and in the urine, &c. He has not found the soda, as represented by former chemists, to impregnate the animal fluids; and this he seems to think might have been concluded *à priori*, because it is admitted on all hands, that almost every kind of vegetable food contains the potash united to some matter destructible by fire, which is not the case of soda;

and that it is as little likely the potash should be altered by digestion, as the muriate of soda itself so constantly taken with our food. It is worthy of remark, that the potash is in much larger proportion in expectorated matter than in the serum of the blood; so much so, that expectorated matter when exsiccated commonly shows signs of deliquescence on exposure to the air.

June 22.—A letter from Mr. T. A. Knight was read, on the relative influence of the male and female on the size and character of the offspring. Contrary to the opinion of Linnæus, Mr. K, considers the female as influencing the size and character, but opposes Mr. Cline's opinion, that large females should be used for breeding; because, although their legs will be longer in proportion to the size of the fœtus, yet their bodies will want the due proportion of depth and thickness, and the animal will be less vigorous and powerful. Thus, for instance, foals of large mares and small horses have the chest thin and narrow, whereas the contrary is the case with those of small mares and large horses. Mules from large mares the author found unserviceable from their want of proportion, and consequently want of strength.

The Society for improving animal chemistry furnished a paper by Mr. Home, on animal secretions. Mr. H. formed some plausible conjectures on the probable effects of electricity in assisting the secretion of blood, serum, albumen, and the other animal fluids. He was induced to this opinion by examining the electric eel, and the immense quantity of nerves which appear necessary to produce the electric power.

Some interesting additional observations by Messrs. Pepys and Allen were read, on the azote disengaged by respiration. The authors in all their experiments on this subject found that a considerable quantity of oxygen was lost in the process of respiration, and that azote was formed; that an animal can breathe oxygen and hydrogen an hour without any inconvenience, but that hydrogen alone occasions sleepiness. The term azote, they observe, is an indefinite name for all gas that is incombustible, irrespirable, and inabsorbable by water; but, from Professors Davy and Berzelius's

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lius's experiments, they conjecture that it is really of a *metallic* origin.

An additional account of M. De Luc's atmospherical electroscope was read, and also some illustrations of his theory of meteorology, developing his opinions of the origin of repeated thunder-claps, clouds, hail, &c., and other meteorological phænomena. The author accounts for the rapid fall of the barometer previous to a thunder-storm, by supposing the existence of some unknown light fluid which ascends in columns at such times. This supposed fluid in his opinion effects various other purposes of atmospherical phænomena.

The Society then adjourned during the long vacation till Thursday the 9th of November next.

FRENCH NATIONAL INSTITUTE.

Analysis of the Labours of the Class of Mathematical and Physical Sciences of the French Institute, for the Year 1807.

MATHEMATICAL DEPARTMENT*.

Astronomy.—The French astronomers, who are now in possession of excellent instruments and methods of singular perfection, have not allowed any opportunity to escape of practising upon these instruments and these methods all the amelioration which reflection aided by long practice can suggest. There were grounds for supposing, that in the construction of telescopes all possible combinations had been exhausted. In fact, the great mirror is necessarily concave, in order to collect under one and the same point all the rays of light which it reflects; but the second mirror may be concave, as in Gregory's telescope, plain as in Newton's, or convex as in Cassegrain's; in short, we may suppress this second mirror as proposed by Lemaire, and so happily accomplished by M. Herschel.

Instead of these four plans, all of which have their advantages and disadvantages, M. Burckhardt has proposed to substitute a fifth, which should have in addition all the merit of facility and of convenience. His small mirror is plain like Newton's: instead of placing it obliquely to the focus of the great mirror, *i. e.* towards the upper extremity of the tube, which renders it inconvenient to observe under many circumstances, particularly in large telescopes, he places it perpendicularly to the axis, and towards the half of its length. In this place the section of the reflected cone of light is a circle, the diameter of which is precisely the half of that of the great mirror: it will therefore intercept a fourth part of the direct rays; but M. Burckhardt remedies this loss by giving a larger dimension to the first mirror. The retrenched cone assumes a reverse position: the rays, instead of uniting, as they would have done, beyond the plain mirror, are collected at an equal distance, but in front, and pass through an aperture made in the centre of the great mirror, in the space which, as we have seen, receives no direct ray, and which is consequently useless for assisting vision. The advantage of this construction consists in reducing the length of the telescope one half, which thereby becomes easier to manage, and less costly. If the diameter of the concave mirror is a little larger, the central part which should have a hole requires no trouble; it is sufficient that the speculum, the only useful part, should receive the curvature necessary for the distinctness of the image; and when it was really a little difficult to render it very exact, we might make up for it, since we have only a single mirror to curve, and because the plain mirror, on account of its dimension being a little larger than in the Newtonian telescope, furnishes easier and more precise verifications. The observer should be placed at the lower part and behind the great mirror, as with Gregory's telescope, which is the most convenient position for following a star continually changing its place. Finally, M. Burckhardt has calculated, by setting out from the measurements of Newton himself, that a telescope of eight metres in focal length, reduced in this way to the actual length of four metres, would have three times more light than a common telescope of four metres, and would have a very valuable advantage over the latter for micrometrical measurements, on account of the double distance of its focus.

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* Drawn up by M. Delambre, Secretary.

Before putting his new idea into execution, M. Burckhardt fairly discussed it. Several objections were started:—the result was, however, that the idea deserved a trial. M. Caroche undertook to make the plain mirror proposed by M. Burckhardt, and to adapt it to a telescope the great mirror of which was two metres in focal length, and the aperture about a sixth of its length.

The invention of Borda's circle, from its exactitude, lightness, and moderate price, forms an interesting period in the progress of modern astronomy. The utility and convenience of this instrument for geodesic operations is universally acknowledged: it is admitted to be superior to every thing for fundamental and delicate researches, in which the necessity is felt of multiplying angles in order to attain the utmost precision. Thus, in order to determine the altitude of the pole, the obliquity of the ecliptic, the equinoctial and solstitial points, the declinations of the most brilliant stars which are not too close to the zenith, and finally for refractions, Borda's circle seems preferable to the largest mural or entire circles which are not repeating. It is therefore doing a real service to extend to new objects the utility of so precise an instrument: we may also employ it in the determination of the hour by absolute altitudes either of the sun or stars. The astronomers who have recently measured the meridian of Dunkirk and Barcelona, have already derived the advantage of thereby regulating their pendulums; they have supposed that in the interval of four or six minutes, during which four or six observations may be made, the altitude increases uniformly in proportion to the interval of time; and thus we may without any risk take a medium between four or six consecutive observations, and treat them, by taking a simple arithmetical method, as we would treat a single observation. M. Delambre, in fact, ascertained that there was no sensible error when the observations regularly succeeded each other; which is most commonly the case. As the contrary, however, may sometimes happen also, he had sought for a method of correcting the small error of supposition and of these various methods; he has only published one, which, however, he had never occasion to make use of. These methods may also be applied to the observation of the distances from a star to a terrestrial object for the determination of the azimuths. M. Burckhardt has contrived a new one, which he discovered by twice differencing the formulæ of the altitudes. The correction of the second differences is proportional to the square of the variation of the horary angle multiplied by a constant. This square may be taken in the table which M. Delambre has given; and immediately we easily determine the correction, having precise results for the hour, notwithstanding the inequalities of motion in the altitude.

In the observations of a star before and after its passage to the meridian, in order to have the meridian height, we may suppose the declination constant when a star or even the sun is observed about the time of the solstices; but towards the equinoxes in particular, we must take an account of the variation in declinations; and M. Delambre has also given on this head a formula of a convenient application to all the planets, and even to the moon. M. Burckhardt now gives another, still simpler, since it merely consists in adding to the mean altitude the motion in declination between the mean instant and the passage to the meridian; but this seems to require more rigorously an equal number of observations before and after the passage, as well as equality among the corresponding horary angles.

The parallax of right ascension requires a second correction when the moon is under observation; M. Burckhardt reduces it into tables of an equally convenient construction and application; he is the first who examined this problem, by means of which Borda's circle will give the meridian altitudes of the moon with the same precision as that of the stars, the declination of which has no sensible motion.

When a star is very distinct, like the sun and moon, it is easy to bring it into the object glass for each successive observation; but when it is a star, we experience greater difficulties: the use of the azimuth circle, intended for these inquiries, is tedious and inconvenient; we may see in the meridian the various methods resorted to by M. Delambre. M. Burckhardt proposes a moveable arc of a circle, which he attaches to the azimuth circle with a screw, and

which

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which prevents the alidada from going from one extremity of this arc to the other, without describing precisely an arc of 180 degrees. In this way the circle is in the vertical of the star; and in order to find it, we have only to give to the circle or to the object glass a vertical motion; but this method would still be insufficient if we had to observe a star by day light, for in this case we might pass far above it without perceiving it.

If the star has a perceptible azimuth motion in order to bring it to the centre of the glass, we shall be under the necessity of slackening the screw, in order to displace a little the subsidiary arc: this attention will neither be long nor troublesome.

This subsidiary arc requires a small change in the form of the alidada; but without in the least changing this form, a simple trace with the crayon upon the azimuth circle, or rather a small spring which should drop in order to allow the alidada to pass, and which should rise when it has passed, would be sufficient for bringing it either to the same position or to a different position of 180 degrees in azimuth.

[To be continued.]

LXXXIX. *Intelligence and Miscellaneous Articles.*

ERUPTION OF ETNA.

Sicily, April 12, 1809.—"Mount Etna burst out on the 26th or 27th ult. in a most tremendous manner. The first great eruption was from the very top. Twelve new craters opened shortly afterwards, about half way down the mountain, and have continued to throw out rivers of burning lava ever since. Several estates have been covered with the lava 30 or 40 feet deep. During the first three or four nights, it was seen very distinctly from this place, and a very large river of red hot lava running down from the crater."

PETRIFIED TORTOISE.

As some men were lately digging in Swanage rocks, on the island of Purbeck, a petrified land tortoise was discovered, seventy feet deep from the surface, in the highest state of perfection; the Rev. Samuel Woolmer being in the neighbourhood, the men brought it to him for his inspection, who being struck with admiration at so great a curiosity, immediately offered them five guineas for it, which they declined accepting, but after exhibiting it about, sold it to a gentleman of Upway, for eight guineas; since which 300*l.* has been offered for it, but refused. It was supposed very probable that its mate might be found near, as the male and female are generally together: upon which further search was made; when after digging some time, another was dug up, but entirely broken in pieces and spoiled.

LIST OF PATENTS FOR NEW INVENTIONS.

To Thomas Noon, of Burton-upon-Trent, in the county of Stafford, for improvements on guns, pistols, and other similar fire-arms, which improvements are applicable to cannon and other large guns.—May 4, 1809.

To Nugent Booker, of Lime Hill, in the county of Dublin, for his new plan for improving and erecting lime-kilns, whereby a very considerable saving is made in fuel, and the lime most perfectly burnt in a short time, which he denominates Grellier and Booker's lime-kiln.—May 9.

To Bartholomew Folsch, of Oxford Street, in the county of Middlesex, merchant, for improvements on certain machines, instruments, or pens, calculated to promote facility in writing.—May 9.

To William Johnson, of Blackheath, in the county of Kent, gent., for his new or improved process for heating fluids for the purposes of art and manufacture.—May 15.

To Edward Manley, of Uffculm, in the county of Devon, for a plough upon an entire new construction.—May 30.

To John Lindsay, (late lieut.-col. of the 71st regiment,) of Grove House, Edgware, in the county of Middlesex, for a night and day telegraph.—May 30.

To Edward Cragg, of Hertford, in the county of Chester, carpenter, and William Cragg, of Old Ford, in the county of Middlesex, builders' agent, for certain new modes of improvements in the making or preparing of salt.—June 8.

To John Frederick Archbold, of Great Charlotte Street, in the county of Surrey, gent., for an improvement in the system of distillation, rectification, and brewing.—June 8.

To Thomas Wells, of Erdington, in the county of Warwick, cock-founder, for a method of making and constructing barrel cocks and water cocks.—June 8.

METEOROLOGICAL TABLE,

BY MR. CAREY, OF THE STRAND,

For June 1809.

Days of the Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.			
May 27	60°	69°	54°	29·64	70	Fair
28	62	71	53	·65	62	Fair
29	60	70	52	·50	64	Showery
30	50	61	50	·89	80	Fair
31	50	63	55	·75	51	Fair
June 1	54	73	50	·42	85	Fair
2	52	57	47	·52	46	Stormy
3	50	63	55	·99	80	Fair
4	55	66	55	·69	51	Showery
5	53	63	51	·34	47	Rain
6	52	66	52	·56	48	Showery
7	52	64	51	·79	62	Showery
8	53	63	51	·65	32	Rain
9	49	59	50	·59	30	Rain
10	50	60	52	·59	33	Showery
11	52	59	55	·86	47	Cloudy
12	59	69	56	30·10	56	Fair
13	60	69	54	·01	61	Fair
14	61	68	55	29·93	58	Fair
15	58	66	54	·90	81	Cloudy
16	56	68	55	·95	85	Fair
17	57	69	52	·78	62	Fair
18	55	67	54	·85	59	Fair
19	56	66	56	·91	82	Fair
20	62	76	62	30·10	91	Fair
21	63	73	63	·26	85	Fair
22	64	69	64	·35	59	Cloudy
23	66	76	62	·31	78	Fair
24	66	74	60	·36	96	Fair
25	56	65	49	·45	71	Fair
26	52	65	50	·38	75	Fair

N.B. The Barometer's height is taken at one o'clock.

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Printed by Richard Taylor and Co., Shoe Lane, London.

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